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AN
ESSAY
ON THE CURE OF
ABSCESSES by CAUSTIC,
And on the TREATMENT of
WOUNDS and ULCERS;

ALSO

A NEW METHOD of curing the
LUES VENEREA.

To which are added,

Dr. HUNTER's Opinion, and Mr. CRUIKSHANK's Remarks on this Method, and on the ABSORPTION in Human Bodies; with ſome Experiments on Inſenſible Perſpiration.

By PETER CLARE, SURGEON.

Curationem tamdiù differunt, ut vitæ planè tædeat inter Empiricorum tormenta & longiſſimas Medicinæ moras protractæ, et ferè leviùs ſit ægrotaſſe quàm ſanari.

Graviora morbis patimur Remedia.

SYDENHAM.

The SECOND EDITION, with ADDITIONS.

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PREFACE.

HAVING ſeen, in a long attendance on the practice of an hoſpital, wounds and ulcers treated by means quite oppoſite to each other, lenient *and* irritating, *I formed an early opinion in favour of the firſt: from which I had remarked that in general the greateſt advantages were derived, and ſeldom, if ever, any miſchief.*

My own practice ſince has confirmed what I there obſerved.

Being perſuaded this method of treatment will conduce much to the benefit of mankind, an attempt to recommend it, I truſt, will continue to meet with a candid reception from the public.

I have heard Mr. Pott *frequently remark, (and I find he has ſince publiſhed the ſame ſentiments in the preface to his* Obſervations on wounds of the head;) "*That many of thoſe gentlemen who came to the hoſpital to finiſh their chirurgical education, were too apt to fix their attention on the operative part of ſurgery only.*" *Operations, he obſerved, would ſeldom occur to them in practice, in compariſon with the common buſineſs of ſurgery,* the dreſſing *of* wounds *and* ulcers, *which would daily fall under their care; he therefore wiſhed them above all things* to endeavour to make themſelves maſters of the healing art.——*I always thought there was good ſenſe in this advice, and I have made it my ſtudy to purſue it.*

I am happy in every opportunity of giving the reſpectful teſtimony of a pupil to the merits of that great profeſſor Dr. Hunter, *whoſe celebrated lectures in anatomy, at the ſame time that they explain the*

the origin of diſeaſes, point out their nature and cure, and have greatly advanced the art of healing.

I cannot think, (as many do,) that preceptors are adequately paid. In my opinion there is a debt remaining where knowledge has been imparted, eſpecially the knowledge by which we live: and gratitude requires that theſe obligations ſhould ever be acknowledged, though opportunities may be too few, and life too ſhort to requite them.

With regard to the latter part of this Eſſay, the Abſorption *of* Calomel from the ſurfaces of the Lips and Cheeks, *my experience and ſingular ſucceſs confirm me every day more ſtrongly in this practice.* Mr. Cruikſhank *having ſeen many patients with me in* Chancery Lane *for ſeveral months paſt, before they had entered on the proceſs, during the courſe of it, and alſo after they were cured, the public are referred to his teſtimony. Before that time, I had*

I had taken the liberty of ſending ſome of my patients to Windmill Street, *requeſting* Dr. Hunter's *and* Mr. Cruikſhank's *inſpection and ſtrict examination of them; and I received from theſe gentlemen very obliging teſtimonies of their approbation of my proceſs. I was induced to act in ſo circumſpect a manner, from an idea ſuch means were proper to be taken by one who wiſhed to conciliate the good opinion and confidence of the public, and from a conſideration, that the beſt cauſe receives additional weight and luſtre from competent and credible witneſſes.*

Having ſelected ſome caſes from the many cures performed ſince the publication of the firſt edition, I have only to ſay theſe will be found faithfully related towards the concluſion of the Eſſay, with vouchers of undoubted credit for ſeveral of them. I have alſo added an engraving of the ſalivary glands, villi of the lips, and internal ſurface of the mouth, *parts with which this mercurial proceſs* *is immediately concerned. I ſhall be ſuſpected here of borrowing from two eminent foreign profeſſors of anatomy; but if their works are not commonly to be procured, it may not be thought impertinent in me to bring them together, and to make this part of their labours more generally known, whilſt my method is thereby rendered more intelligible to the greater number of readers.*

I would aſk the faculty one queſtion. If mercury can be introduced into the circulation by the method here recommended, and the diſeaſe more expeditiouſly cured, is it not preferable to the conveying it into the ſtomach and bowels?

I apprehend no perſon of any candor will anſwer it in the negative, againſt common ſenſe and anatomical demonſtration. But an oppoſition to this proceſs from our profeſſion, I own, may probably ariſe from two circumſtances; a diſinclination to adopt a new practice, and an appre-

apprehenſion (well or ill founded) that patients may attempt hereafter by this method to cure themſelves.

I have republiſhed Dr. Hunter's Remarks *on this ſubject, which, though conciſe, are much to the purpoſe.—I copied them from the Doctor's own hand writing in which he gave them to me, and further favoured me with his approbation of this practice, in a ſimilar mode of communication.*

Mr. Cruikſhank *has enlarged his Letter ſo much, that my Eſſay appears ſmall indeed. I do not however think any apology neceſſary on that account, from either of us; as the inquiſitive and intelligent reader, I am perſuaded, will not think the former too long, nor yet the latter too ſhort.*

AN ESSAY On the CURE of ABSCESSES.

On the nature of Abſceſſes, and their formation.

AN abſceſs is a collection of matter commonly ſeated in the adipoſe or cellular membrane, the immediate connecting ſubſtance betwixt the muſcles and the ſkin, and which connects the fibres of muſcles themſelves. The formation and progreſs of an abſceſs is uſually attended with great pain and fever. Abſceſſes are rarely formed without rigors, which ſeem to be the ſymptoms of a conſtitution labouring under ſome morbid attack. For

the

the moſt part they advance gradually towards the ſkin. Nature here intimates to the ſurgeon what line of conduct he ought to purſue; either to make an external aperture to give vent to the matter, or to leave that to her, which ſhe will effect in general ſooner or later, according to the ſtate and circumſtances of the conſtitution. How very improper then, in moſt caſes, muſt be any attempt to repel abſceſſes, or oppoſe her ſalutary intentions! Yet this is often done at the deſire of the timid patient, who wiſhes to avoid the pain and trouble of a wound; but is obliged at laſt to ſubmit to Nature, who will have her courſe, and, when left to herſelf, even in ſuch circumſtances, has been known to perform excellent cures.

Every abſceſs is attended with inflammation, but every inflammation does not terminate in an abſceſs: now an abſceſs is frequently the criſis of ſome internal diſeaſe, and muſt not be repelled; but a ſimple inflammation, from an external cauſe, often is, and may be diſperſed with the utmoſt propriety.

Nature is aſſiſted in her purpoſe of ſuppuration by emollient poultices and fomentations, when the ſkin is tenſe and painful; when not, a warm plaiſter is as effectual, the patient at the ſame time being ſupported by a nutritious diet and cordial medicines, eſpecially if the pulſe and other ſymptoms indicate the want of them.

The ingredients of a poultice ſhould be boiled together and ſtirred till it quits the ſides of the veſſel and adheres to the ſpoon: it ſhould have an unctuous ſurface, and be applied warm.

Fomentations are of peculiar ſervice, becauſe their ſubtle vapours penetrate the pores of the ſkin, which, being very minute, refuſe the admiſſion of the groſſer fluids, oil or water; for this reaſon fomentations give relief when other applications fail.—The ſtupes ſhould be applied as dry and warm as poſſible.

When the fever is high, adding to it would anſwer no good purpoſe; neither muſt it in general be abated by evacuations, ſince the fever being briſk, the matter is ſooner formed, for till ſuppuration is fairly begun, the patient ſeldom meets with much alleviation of his pain.

Mr. Sharp, in his Introduction to a Treatiſe on the Operations of Surgery, page 10, juſtly obſerved, "Bleeding ſometimes advances the ſuppuration of an abſceſs, but this practice is to be followed with caution." If inflammation is very violent, there is danger of the parts running into mortification. Bleeding, by moderating this inflammation, may promote ſuppuration, and do good.

Prudence directs us not to be ſatisfied with a partial collection of matter, (leſt there ſhould be a ſecond depoſit), but to wait till the whole is compleatly formed, which is known by a palpable fluctuation on compreſſing it, and by the fullneſs and thinneſs of the ſkin. Veſications of the cuticle often appear at this time, which portend an approaching aperture: this is the critical ſeaſon for making it, unleſs it ſhould be deemed more adviſeable to leave this opening to be effected by Nature.

As there is no rule without an exception, ſo in abſceſſes we muſt not always ſtay till the tumour is fully ſuppurated; but if we find it *receding*, and the patient's health affected by the abſorption of the peccant humours into the habit, we are fairly warranted in making an earlier opening.

Of opening Abſceſſes.

WHEN an abſceſs breaks of itſelf, there is uſually a ſmall orifice, which does not prove ſufficient to diſcharge the contained matter, and heals before it has produced the deſired effect; a circumſtance which made it expedient for the profeſſion to deviſe the means of opening abſceſſes more freely, that there might be no confinement of the matter within the ſkin. For this purpoſe ſome practitioners prefer the knife, others the cauſtic. The advocates for the former dwell much, and with ſome reaſon, on the utility of preſerving the ſkin as much as poſſible, which is done by a ſimple diviſion of it, when it recedes only for a time, reſuming its former ſtate as the wound heals, and frequently without loſing many of its fibres. This occaſions leſs ſcar than the cauſtic, which totally deſtroys a certain portion of the ſkin. The knife however has not thoſe advantages it ſeems at firſt ſight to poſſeſs; for though the opening made by the cauſtic may ſeem large, yet in moſt caſes the neighbouring ſkin will at laſt be drawn in, will cover the greateſt part of the ſore, and very little new ſkin will be wanted.

Though every judicious and humane practitioner will wait (unleſs it is receding) till the abſceſs is fully ſuppurated, not only that the ſkin may have loſt its painful ſenſibility before he makes the inciſion, but becauſe opening it ſooner does no good ſtill it may not be a matter of indifference which way it ſhall be opened; there is ſo great dread of the knife in ſome minds, that they cannot be perſuaded to ſubmit to it; many of the profeſſion therefore have ſubſtituted the cauſtic as a leſs terrifying and much eaſier mode of effecting the needful aperture.

There is no better cauſtic than the paſte of the London Diſpenſatory. "Let an orifice be cut in a piece of ſticking-plaiſter, nearly as big as you mean to make the eſchar, which being applied to the part, the cauſtic muſt be laid on the opening, and preſerved in its ſituation by a few ſlips of plaiſter placed round its edges, and a large piece over the whole;" a bandage is likewiſe a neceſſary ſecurity; notwithſtanding which the cauſtic uſually ſpreads one third beyond the limits preſcribed by the plaiſter.

The cauſtic being placed on the tumour, and continued there three or four hours, cauſes death in or a mortification of a certain portion of the ſkin and integuments. This operation is attended with different degrees of pain, according to the condition of the ſkin, which, in a cool and uninflamed ſtate, bears the cauſtic much better than when in a contrary one; for at that time the ſlighteſt preſſure of the finger hurts exceedingly; it may be neceſſary even in ſuch a ſituation however to employ the cauſtic, but humane ſurgeons now generally mix it with opium [*]. Cauſtics ſometimes give but little pain, have often been called *drawing plaiſters*, applied as ſuch, and paſſed undiſcovered. Thoſe ſurgeons who have joined opium with their cauſtics affirm, that their patients have fallen aſleep during the operation [†].

A principal advantage of the cauſtic is the opening it makes for a free and complete diſcharge; and this treatment is very rarely followed by ſinuſes, jagged lips, or callous edges, which often appear after the uſe of the knife, even in a good habit of body.

A puncture with the lancet ſometimes diſcharges the contents of an abſceſs, and effects an eaſy and perfect cure; but the leſs certainty of its ſucceſs makes me give a preference to the larger opening by cauſtic.

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[*] See Edinburgh Medical Comment. Vol. I. N°. 4.

[†] See Mr. *Elſe*'s Eſſay.

Sinuſes are frequently cured by compreſs and bandage, eſpecially when aſſiſted by proper medicines. Theſe failing, recourſe is commonly had to inciſion, which ſurgeons make freely at firſt to expedite the cure, and to prevent the diſagreeable neceſſity of a repetition.

For this purpoſe, Mr. Sharp and Mr. Pott recommend the knife, in preference to the ſciſſars, which they condemn in the ſtrongeſt terms, on account of their pinching at the ſame time that they cut. Beſides, the edges of wounds, thus divided, tumify, are generally painful for ſeveral days, and indiſpoſed for healing. The cauſtic-paſte is, in my opinion, much eaſier, and as effectual as inciſion.

Counter and depending openings, judiciouſly made, are often of great ſervice where there are ſinuſes, and preſerve ſkin. It is the practice with ſome ſurgeons to cut away an oval or circular piece of the ſkin in abſceſſes, after having

ing firſt made a longitudinal inciſion; which is indeed recommended by Mr. Sharp, "where there is much diſco-"loured ſkin;" but, though greatly diſcoloured, it will often recover, or, if much diſeaſed, will periſh of itſelf: this method therefore can ſeldom be neceſſary; and ſeems, notwithſtanding ſuch reſpectable authority, not to have ſufficient reaſon for its ſupport.

When the cauſtic is removed, the matter ſometimes burſts forth; when it does not, it may be ſet at liberty by the introduction of the lancet, which is now not at all felt, if the cauſtic has duly performed its office. The eſchar commonly ſeparates in five or ſix days; to effect which purpoſe, ſome ſurgeons apply hot terebinthinate dreſſings, conceiving them to draw, and mechanically help the ſeparation; when in truth this event is produced by the granulations underneath, which ſhews it to be the act of Nature, external dreſſings availing little; but internal medicines, by adding vigour and impulſe to the circulation, are of uſe on that principle, and promote the ſeparation of the eſchar.

Some practitioners have obſerved, that much miſchief is often done by the improper mode of dreſſing wounds made by inciſion, either by diſtending them with large and hard doſſils, or by dreſſing them too ſuperficially; but the ſore from a cauſtic is liable neither to one abuſe nor the other, and the cure advances of itſelf, requiring the leaſt aſſiſtance imaginable. This ſurely is an argument ſtrongly in favour of the latter. Another of as great weight is, that a cauſtic, of the ſize of a ſixpence, laid on a depending part, will effect a cure as certainly as an inciſion of ſeveral inches of the ſkin, which I have known frequently, (as have others of the profeſſion), particularly in large abſceſſes near the rectum, called ſometimes *Incomplete Fiſtulæ*; as well as in other parts.—

Mr. Sharp ſays, "When iſſues are "made, or bones expoſed, the eſchar "ſhould be cut out immediately, or the "next day."—This operation muſt give great pain to the patient, and ſome to the ſurgeon alſo, if he poſſeſſes thoſe tender feelings, for which Mr. Sharp was always eſteemed, as well as for his judgement and ſingular dexterity in the operative parts of ſurgery.—The eſchar however need not be cut away.—One or more kidney or French beans, with a proper compreſs and bandage, will adapt themſelves, immediately after the ſeparation of the eſchar, with very little pain; and, on account of their form, are preferable to peas, which penetrate ſo deep into wounds, as not only to be painful, but injurious.

Chirurgical writers deſcribe wounds and ulcers as going through three ſtages in the courſe of their cure,—*digeſtion*, *incarnation*, and *cicatrization*, to which ſome add mundification, placing it betwixt the firſt and ſecond, and ſuppoſing it equally connected with them both.

Of Digeſtion.

IN the firſt ſtage *digeſtion*, (which is the formation of a certain quantity of good pus on the ſurface of a ſore), oily dreſſings are moſt in uſe, and theſe ſhould be applied warm, but never hot; it being an eſtabliſhed poſition, to the honour of modern ſurgery, that the moſt mild applications anſwer this purpoſe beſt. It is perhaps doubtful whether there are any ſuch applications as can promote digeſtion, but theſe may be conſidered in effect digeſtives, as giving no interruption to Nature in this great and important buſineſs, whilſt painful dreſſings, conſiſting of hard doſſils, and loaded with corroſive powders, (falſely called Digeſtives), defeat the very aim and deſign they were intended to promote. Digeſtion is an act of the conſtitution, and when the latter is in a good ſtate, the former advances properly. When the *vis vitæ* is defective, internal medicine is required, and does infinitely more good than any external application whatſoever.

Of Incarnation.

THE ſecond ſtage is that of *incarnation,* in which the wound fills up with granulations of fleſh, and, when healthy, has a florid aſpect, which is an excellent ſymptom. When theſe are luxuriant, and riſe above the edges of the wound, it has been uſual to call ſuch an appearance a Fungus, and to apply corroding medicines to reduce it to a level with the ſkin; but theſe commonly give great pain, and protract the cure. Happily they are now laid aſide in a great meaſure, authors and practitioners of the greateſt abilities declaring, that dry lint with moderate preſſure is ſufficient to reſtrain ſuch luxuriancy.

Mr. Sharp very ſenſibly and humanely obſerves, with regard to the fungus in a wound made by a ſharp inſtrument, where there is no indiſpoſition of body, "that dry lint only is generally the beſt dreſſing: at firſt, it ſtops the blood with leſs injury than any ſtyptic powders or waters; and afterwards, by abſorbing the matter, which in the beginning of ſuppuration is thin and acrimonious, it becomes, in effect, a digeſtive. During incarnation it is the ſofteſt medium that can be applied betwixt the roller and tender granulations; and, at the ſame time, is an eaſy compreſs upon the ſprouting fungus."—This is perfectly lenient, and compriſes no ſmall part of the proceſs of healing ſores in few words.

Mr. Freke, in his treatiſe on the Art of Healing, publiſhed in the year 1748, was the firſt writer who oppoſed cauſtic and corroding applications to fungus in ulcers, and recommended warm poultices to promote perſpiration in the part affected; remarking, that callous edges are beſt ſubdued by relaxing means: in this mild doctrine he is ſupported by Dr. Hunter, Mr. Pott, and many modern practitioners, who have laboured to eſtabliſh it for the greater immediate eaſe as well as future benefit of their fellow-creatures. Formerly ſurgeons pared away callous edges with the knife; and what was the conſequence? After the moſt excruciating pain, the calloſities returned; the cauſe, namely the bad habit, remaining, and an irritation being kept up inimical to healing, which, on the contrary, is beſt advanced by the perfect tranquillity of the part affected.

Mr. Sharp ſays, Calloſities are to be touched for a few days with the lunar cauſtic, or *lapis infernalis*, as leſs painful than the knife; and the eaſieſt method of removing *looſe lips* (though on other occaſions he prefers the knife) *is cutting them off with the ſciſſars.* P. 31.

When excreſcences have ariſen in *venereal* ulcers, he ſays, " he has pared them with a knife, but the flux of blood is ordinarily ſo great, that he does not recommend the method, and rather prefers the eſcharotics. Thoſe in uſe are, the vitriol, lunar cauſtic, the *lapis infernalis*, and more generally the red precipitate powder; but even in this caſe, he does not think that powder the beſt remedy: for he ſays, though it is always an eſcharotic, yet the *pulv. angelicus*, which is a compoſition of precipitate powder and burnt allum, eats deeper, and, he thinks, is preferable to the precipitate alone." P. 34. Surely ſuch caſes demand rather a proper correction of the habit.

The carious ulcer with fungus cannot be cured without a previous exfoliation of the diſeaſed bone. See the ſame author, p. 44.

Mr. Freke ſays, page 107, "All "writers on this ſubject have agreed to "ſubdue funguſes only by drying vitriolic or cauſtical applications; whereas "my method is quite contrary to what "is univerſally laid down; which is, "that greaſy or mucilaginous relaxing "medicines do ever create funguſes. "I readily own the propoſition to be "true; for which reaſon I make uſe of "a muci-

“ a mucilaginous cataplaſm to relax
“ theſe ſchirrous diſeaſed parts, and
“ thereby make them diſcharge their
“ enemy. And in theſe caſes I let the
“ fungus increaſe as much as it will.”

Again, page 110, “ I never think of
“ deſtroying it by vitriol or the like, but
“ rather chooſe to give a full looſe to
“ the parts, finding them healing faſter
“ from the edges when the ſtrangula-
“ tion has been taken off, by ſweating
“ out that which occupied ſpaces which
“ did not belong to it, rather than by
“ art, cropping off the top of the fun-
“ gus by vitriol, &c. and thereby forc-
“ ing it to heal, whilſt it remains cal-
“ lous and diſeaſed.”

Theſe obſervations, and many others, the reſult of long experience in St. Bartholomew's Hoſpital, as well as an extenſive practice elſewhere, ſhew Mr. Freke to have been a man of a penetrating genius and ſound judgement. They are his own original ideas, and form the baſis of the modern mild approved practice, which without doubt will ſtand the teſt of future ages.

Oil, either in its liquid form, or modified with wax or diachylon, is an univerſal and proper dreſſing for ſores.

Healing is the act of the conſtitution, and depends on internal impulſe, not on the ſpecific quality of an unguent, which ſerves principally to keep the part moiſt, and to defend it from the injury of the external air. Oil is artificial pus, which laſt, when of good quality and conſiſtence, protects and comforts the tender wound; when acrid, it cauſes pain and miſchief; in which caſe oil mixing with the matter, and correcting its pernicious quality, gives eaſe. When the pus is mild, yet too redundant, dry lint abſorbs it, keeps the ſore clean and eaſy, and may be continued till the diſcharge becomes ſo ſmall as not to moiſten the lint ſufficiently to prevent its adheſion. This ſhould be laid light and thin on the middle, not covering the edges of the ſore.

Some may be induced to remark, that the doctrine here laid down, with regard to ointments, *viz.* that they are ſo ſimple, ought not to be divulged, as it may encourage improper perſons to interfere in caſes of ſurgery, to the injury of the public and the profeſſion. The latter can never be hurt by ſuch pretenders, ſince they cannot judge of the good or bad condition of wounds, and therefore they can yield their patients no ſatisfaction. Certainly, in matters of phyſic and ſurgery the merit conſiſts more in the judicious application of remedies, than in the remedies themſelves.

Poultices are exceedingly uſeful where there is tenſion and pain in wounds, as they moiſten and relax the parts, and where the diſcharge is acrid, they abſorb the offending matter, and by their warmth and conſiſtence give eaſe, aiding Nature in her healing intentions.

The fears of the patient have often prevented inciſions, particularly in abſceſſes of the breaſt, the female ſex being diſpoſed to try every method rather than endure the knife; and ſuch caſes have ſometimes happily ſucceeded by the repetition of emollient poultices cauſing an opening; which, by gentle preſſure and patient perſeverance, has effected an entire cure, thereby encouraging the humane ſurgeon to follow the example, and often to forbear the uſe of inciſion.

Heretofore ſurgeons were, with more juſtice, dreaded for their operations and painful dreſſings, than perhaps revered for their ſkill; antient ſurgery reſembling modern farriery, being "coarſe, and loaded with a farrago of external applications, ſome of which were unneceſſarily painful." Patients were then confined long, through injudicious treatment; and wounds (which would have healed, if left to themſelves or treated with lenity) proved inveterate and obſtinate through precipitate, vitriolic, and other ſtill ſtronger corroſive dreſſings. Thus ſores were rendered phagedenic, inflamed and ill-conditioned, from which ſtate they were at length retrieved by the removal of noxious irritating means, and by a recourſe to the emollient ſyſtem *.

Formerly, whenever any enlarged glands protruded in wounds, it was the practice to attack them with the moſt powerful eſcharotics; and where theſe failed, recourſe was had to extirpation by ligature, or the fingers, as ſome now living have cruelly experienced. Theſe glandular appearances are conſidered by modern ſurgeons as no unfavourable ſymptoms, and often form the baſis of a cure when treated with lenity. They are frequently to be ſeen in inguinal and other abſceſſes.

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* I have lately ſeen a ſore in the leg, which at firſt was only a pimple, and in two months became deep and extended from being dreſſed, almoſt every day, with coarſe red precipitate. On changing the application to bread and milk poultice, the patient was cured preſently.

We cannot adduce a ſtronger proof of Nature's propenſity to heal, than the inſtance of the great difficulty with which a pea is retained in an iſſue, or a tent in a wound; where, in ſpite of the utmoſt exertions from preſſure and bandage, Nature oft prevails, throws out the offenſive extraneous body, and heals the orifice.

In like manner will wounds incarnate in defiance of mal-practice. Nature one day puts forth her granulations of fleſh, which are the proper materials for filling up a ſore; the next, perhaps corroſive medicines are applied to deſtroy theſe luxuriant granulations, too often falſely called Fungi, or proud fleſh; and thus is Nature counteracted in her ſalutary endeavours. The abſurdity of ſuch treatment is evident to every unprejudiced obſerver.

Such formerly was the practice; the modern ſurgeons in general have more wiſdom and humanity; they know the eaſieſt dreſſings are the beſt, and that Nature will do the buſineſs herſelf, if not interrupted by thoſe who think they can accompliſh every thing without her aſſiſtance.—Mr. *Freke* ſays, page 126, "A ſurgeon is but a ſcavenger to Na-"ture: for, if the various parts of the "blood did not ſupply the many loſſes "of the body, what could the ſurgeon's "art do for his patient?"

What the antients endeavoured to effect by painful eſcharotics externally applied, the wiſeſt of the modern profeſſors accompliſh by internal, invigorating medicines; correcting the habit; knowing that to be the only rational method of cure which ſtrikes at the cauſe of the evil; for, that once removed, the effect ceaſes.

Cicatrization.

Cicatrization is the laſt ſtage, and compleats the cure.

Sores are covered over, firſt, by the elongation of the ſurrounding old ſkin, which is drawn in towards the centre of the ſore; then, by the formation of the new; and ſometimes from different ſmall points (like iſlands) in the wound, which in this caſe cicatrizes faſt.

An even cicatrix is much to be wiſhed, and (except in caſes of burns and ſcalds) is often eaſily obtained by proper bandage. In common inciſed wounds, where the habit of body is good, the cicatrix is formed without any trouble, whilſt in ulcers of long ſtanding this finiſhing part of the cure takes up much time, and requires ſome deliberation. In theſe ſort of caſes deſiccative (drying) topical applications are generally condemned, fatal conſequences having ariſen from the ſudden ſtoppage of habitual diſcharges. The general ſenſe of the profeſſion, and indeed of mankind at large, is, that Nature ſhould take her own time to effect this her laſt act in the cure of every wound; the preſumption will then be, that ſuch ſores will not break out again. Some ſay it is owing to the application of drying powders and healing ointments, commonly uſed to expedite the cure, that newly-healed wounds too often open again; an accident which, they ſay, the ſurgeon attributes to a very common, though not always a true cauſe, the bad conſtitution of his patient.

To prevent the return of ſores, eſpecially old habitual ones, iſſues near the part affected are uſually and ſtrongly recommended; I have often known them recurred to without ſucceſs; all the artifice of ſurgery could not keep them open any length of time, but Nature had recourſe to her old drain; happily indeed for the patient, if we are to form our judgement from common ſenſe, reaſon, and experience.—*Ab extrà intùs redire, malum eſt.*

In my opinion, an equal bandage, which juſt properly ſupports the parts, eſpecially when the ſores are in the lower limbs, is the beſt ſecurity againſt a relapſe.

Critical Remarks on precipitate and other Dreſſings; with curſory Obſervations on ſome late Improvements.

MR. Sharp, in his *Introduction to a Treatiſe on the Operations of Surgery*, page 32, ſays, "the precipitate is a digeſtive, when mixed "with any ointment;" that being, in his opinion, much leſs painful and corroſive, than when ſprinkled on a ſore in powder; for he ſays, "as it is a ſtrong "eſcharotic, much of it can never be "uſed without making a ſlough, and "therefore continuing it day after day "will be making a ſucceſſion of ſloughs, "keeping under and deſtroying the "little granulations of fleſh, which in

“ their growth would elevate and puſh
“ off the firſt ſlough.”

Hence it ſhould ſeem that Mr. Sharp diſapproved of precipitate dreſſings, as contrary to the principles of healing; yet he has not entirely diſcarded them.

Mr. Sharp raiſes an objection to the cauſtic on this ground, “ that we cannot
“ have the advantage of dreſſing
“ properly till the ſeparation of the
“ ſlough, which often requires a conſiderable
“ time, ſo that the cure muſt
“ neceſſarily be delayed.”

The ſame gentleman however ſays, in page 19, “ Sloughs are flung off by the
“ ſprouting fleſh underneath, which fills
“ up the cavity at the ſame time that it
“ diſcharges the eſchar.” The cure then cannot ſurely be ſaid to be delayed.

He further remarks, “ The pain
“ of burning continuing two or three
“ hours, which a cauſtic uſually takes
“ up

" up in doing its office, draws ſuch a
" fluxion on the ſkin, as ſometimes to
" indiſpoſe it very much for healing
" afterwards."

Daily experience does not confirm this opinion, ſince no wounds heal more kindly than thoſe made by the cauſtic. Mr. Elſe, a very diſtinguiſhed operator, and ſurgeon to St. Thomas's hoſpital, recommends the cauſtic for the radical cure of the hydrocele, in preference to inciſion, and relates ſeveral inſtances of it's ſucceſs.

Leſt the writer's meaning ſhould be miſunderſtood by thoſe who are not of the chirurgical profeſſion, and he ſhould appear an advocate for and againſt the cauſtic, he is deſirous of offering a few words by way of explanation. He approves of the cauſtic paſte as the means of opening many abſceſſes, but objects to cauſtic and corroſive dreſſings as unneceſſarily painful, and highly injurious to almoſt every open wound. In ſome caſes perhaps inciſion is preferable, in others the cauſtic. The latter will always find many friends amongſt the timorous, who will not be diſappointed in the uſe of it. It is never applied to abſceſſes about the face, for an obvious reaſon.

On Healing by the Firſt Intention.

IF there needed any further proof how little neceſſary the tribe of healing ointments are in good conſtitutions, we might inſtance the caſe of freſh wounds, where there is a ſimple diviſion of the ſkin and integuments, the lips of which being brought into contact with each other, and retained ſo by bandage, will heal in few hours by *the firſt intention*, without applying any remedy to the part, the blood alone is here eſteemed the beſt medicine and ſufficient for the purpoſe, preferable to all the ſolutions of gums in ſpirits, called Balſams, which many are fond of uſing; they appear to me only to give the patient pain, without rendering him any benefit in return. Mr. Freke indeed recommends the uſe of balſams to lacerated wounds, "as a varniſh to cover ſuch ſmall filaments, or lacerated parts of the wound, as the air without it would immediately take hold of and mortify." P. 128.

Inſtances of union by the firſt intention are ſeen every day, and often very ſtriking ones. I ſhall mention a very ſingular one.—-During my apprenticeſhip, a youth, about twelve or thirteen, living at the Dog Tavern, Garlick-hill, had the misfortune to fall upon the edge of a caſe knife, which penetrated the left lobe of the lungs, the air from which nearly extinguiſhed the flame of a candle. The hæmorrhage from the wound was conſiderable, till Mr. Pott placed the lad on his back, when the bleeding ſtopped. It was dreſſed lightly with dry lint; but fearing it might bleed internally, he made an unfavourable prognoſtic, and was moſt agreeably ſurpriſed to find his patient had no bad ſymptoms. He recovered in leſs than a fortnight; whence it was concluded the balſamic quality of his blood uniting the wound by the firſt intention had effected the cure. He was bled at firſt, to guard againſt the ſymptomatic fever.

On SUTURES.

OF all the modern improvements in ſurgery, none pleaſes me more than the general diſuſe of needles and ligatures in freſh wounds; they generally did a great deal of miſchief. There ſtill are caſes, however, where one ſuture or more are abſolutely neceſſary; as in wounds of the lip, ſcalp, and other parts.

Where the lips of wounds can be brought nearly or quite into contact with each other, adheſive plaiſter and retentive bandage (abſurdly called *the dry ſuture)* are ſufficient, and ſave the patient an infinite deal of pain and trouble.

It is well known, ſutures are only temporary, and break away as the wound digeſts; are introduced with much pain, exciting inflammation, and ſometimes fever, whereby the cure is protracted.

Even the operation for the hare-lip may be performed without needles, by means of adheſive plaiſter and a bandage, the threads of which interſect each other, drawing in oppoſite directions, bringing the divided edges into approximation, and retaining them in that ſtate. The inciſed lips are to be kept in their due poſition, firſt by croſs ſlips of plaiſter, and finally ſecured by this ſort of bandage, which Dr. Hunter always exhibited in his lectures. Mr. William Deas, ſurgeon to the Dublin hoſpitals, alſo propoſes this method. See Medical Commentaries, N°. 19; where Mr. Deas has likewiſe publiſhed a cure of a cancerous lip by the ſame means: both which practices Dr. Hunter recommended to his pupils more than twenty years ago.

The method of taking up the arteries with needle and ligature after amputations, was ſubſtituted in lieu of cauteriſing or ſearing the veſſels with a hot iron, the horror and pain of which is beyond deſcription. The former, though it gives pain, and is mild when compared with the latter, is ſo very acute, and attended ſometimes with ſuch bad conſequences, that the profeſſion are humanely endeavouring to ſubſtitute ſome eaſier method in its ſtead. Hence the external uſe of the *agaric*, ſtyptic tinctures and powders without number, which are daily tried with different degrees of ſucceſs; but ſometimes failing, and thus endangering lives, they muſt ſoon be diſcarded. We, however, may hope to meet with ſomething effectual for this purpoſe, ſince we are daily informed of freſh diſcoveries.

The tenaculum is now much uſed, and has the advantage of the needle and ligature, which give conſiderable pain by including the nerve; no wonder therefore that the tenaculum is coming more into practice. This inſtrument has been long made uſe of for the ſmaller arteries after amputations, and is now found to ſucceed even in the larger.

Obſervations on the uſe and abuſe of the Bougie.

A Young gentleman, labouring under a continual diarrhæa, which had reduced him to the laſt ſtage of an hectic, ſent for me, on the preſumption it might prove a caſe in ſurgery, by the deſire of the medical gentlemen who attended him. The quantity of water by the urethra being very ſmall, made us ſuſpect there was a communicating aperture betwixt the urethra and the rectum. Having ſatisfied ourſelves as to this particular,

ticular, and proved beyond a doubt that it was ſo, we paſſed the ſilver flexible catheter, and drew off more urine than had been diſcharged by that paſſage for ſome time before. It being deemed neceſſary an inſtrument ſhould be left there to bring away the water at all hours, we ſubſtituted the hollow bougie, which gave no pain, and afforded Nature an opportunity of healing the wound by taking away the preventing cauſe, the inceſſant irritation from the ſalts in the urine. He was cured in a month, and recovered his former health and vigour. The bougie in this inſtance, as in many others, was very beneficial; yet it may not be amiſs to obſerve, that even in the hands of the moſt ſkilful ſurgeon it ſometimes proves exceedingly dangerous; what muſt it be in the hands of the patients themſelves? I have known the worſt conſequences from paſſing even the moſt mild bougie in *aged* perſons labouring under a ſuppreſſion of urine; how great then muſt the riſque of that patient be who ſhould preſume to paſs a bougie himſelf? thinking to remove this complaint by violence, inſtead of eaſy and gradual dilatation, which is the proper mode of uſing it. Two caſes of this kind came within my knowledge; one was that of a perſon of thirty years of age, who, inſtead of the ſurgeons bougie, made uſe of a common wax taper, which excited pain and inflammation; he deſiſted however from the practice before he had done himſelf any conſiderable miſchief: the other that of a gentleman of threeſcore, who, in conſequence thereof, unhappily died of a mortification.

Obſervations on the treatment of compound fractures.

FRACTURES are called *compound*, when there is a wound in the external integuments made by the fractured extremities of the bone: when the fracture is attended with a wound not made by a protruſion of the bone, it is called *complicated*; a caſe by no means ſo dangerous in general as the compound fracture. The hazard is judged to ariſe principally from the admiſſion of air into the injured part; the modern method of cure is therefore to exclude the air as much as poſſible, and by this method to reduce the *compound* to the ſtate of a *ſimple* fracture. This is Mr. Freke's opinion, which he gives in the following words, page 129. "It is owing to the air that a compound fracture differs ſo greatly from "a ſimple one: for, though the bones "in a ſimple fracture be ever ſo much "lacerated, it ſeldom or never, with

“ proper care, fails of doing well; but
“ if the ſkin be broken ever ſo little, if
“ the air be admitted to ſome extravaſated blood, it makes a terrible difference betwixt one fracture and the
“ other, which every knowing practitioner finds there is, from the cauſes
“ before given.”

By the excluſion of the air many compound fractures, of late years, have been cured in a ſhort time, and with little trouble; recourſe having been had to phlebotomy and proper medicines; ſuch as opiates, bark, &c.

The common external remedies in theſe caſes have been oily relaxing liniments and poultices, which requiring frequent renewal, have expoſed the ſore to the bad influence of the air, and encouraged too copious a diſcharge; in time, reducing the patient's ſtrength exceedingly; inſomuch that the bark, and other reſtorative cordials, given with the greateſt propriety and moſt liberal uſe, have not been able to prevent the patient from ſinking.

Senſible of this circumſtance, both Engliſh and French ſurgeons have, for ſome years paſt, adopted aſtringent and ſpirituous embrocations in compound fractures, moiſtening the dreſſings and bandage frequently with theſe, and forbearing to renew them as long as poſſible, that the external air might not gain admiſſion; they had ſeen, no doubt, the ill conſequence of ſuch practice, and how much the patient's ſafety depended on the former precaution. Sinuſes, ſo common in theſe caſes, have been hereby prevented, as well as long confinement and delay; for it is well known that the callus, whereby the fractured extremities of bones are united, cannot form whilſt the matter lodges, and cauſes a continual interruption to Nature in her important work.

It has been generally remarked, that the cure of compound fractures does not ſucceed ſo well in the London hoſpitals as in the country, where the air is more pure and conducive to health. Another cauſe aſſigned, is the free and intemperate way of life of thoſe, who, by indulging themſelves too much in ſpirituous liquors, impair their conſtitutions, and, when an accident befals them, their blood and juices are too much vitiated to admit of a cure: hence a mortification enſues; and even the bark, that ſovereign remedy, fails in its wonted relief.

A remarkable inſtance of the good effects of this method of treatment occurred ſome years ago in the perſon of Mr. Pott, who unfortunately met with a compound fracture of his leg by a fall from his horſe, at the time of my living with him, and of courſe I was a witneſs to the proceedings in the cure. The accident was treated like a ſimple fracture, and the air excluded.

His age was betwixt forty and fifty, and his conſtitution good, the wound healed without any ſinus, and the callus was compleatly formed in eight or ten weeks. If relaxing means had been uſed, ſuch as poultices, which are here improper for the reaſons before mentioned, it is a queſtion if this gentleman's many ſubſequent ſervices would not have been loſt to the community.

Previous to his own accident, Mr. Pott had treated caſes of this kind in the hoſpital with great ſucceſs; amongſt which was that of a boy of eight or ten years of age, who had a fracture of the radius, which protruded through the ſkin. It was bound up as a ſimple fracture, and the wound was healed in a fortnight; a circumſtance which gave Mr. Pott and his pupils great pleaſure, they having intereſted themſelves much in the event.

Two other caſes in private practice have ſince fallen under my care, and were ſucceſsfully treated in this way. One of them was a gentleman of the Stamp Office, forty years of age, temperate, and of a ſpare habit. His leg was fractured, and did well in eight weeks without any bad ſymptoms. The other was a gentlewoman of fifty, and of a groſs habit, who recovered in twelve weeks by the free uſe of bark and opiates. Here the ſymptomatic fever ran very high at firſt.

Theſe patients were laid on their ſide with the knee bent, the poſition recommended by Mr. Pott, and alſo by Mr. William Sharp, a ſurgeon of great ſkill and humanity, who, in *An Account of a new Method of treating fractured Legs*, has favoured the public with plates of his uſeful ſplints.

By this poſition, the muſcles are entirely relaxed; in the old way, they were always upon the ſtretch, and in a ſtate of action, which occaſioned frequent and ſudden ſpaſms, attended with pain and reſtleſneſs, thereby diſtreſſing the patient and retarding his cure.

I was ſome time ago favoured with the caſe of a compound fracture of the fore-finger of a ſmith's apprentice, in the pariſh of St. Clement Dane in the Strand. The bone was broken obliquely, ſo that the two extremities formed ſharp points. The divided part was ſuſpended only by a very ſmall portion of the ſkin; and being inſtantly replaced in a proper poſition by an eminent ſurgeon in the neighbourhood, defended by plaiſter, ſplints, &c. by the excluſion of the air, and the happineſs of a good conſtitution, the uſe of the finger was in a great meaſure reſtored in a few weeks. There being at firſt, all but an entire ſolution of continuity of the ſkin, this caſe would have been deſpaired of by ſome who have leſs faith, because they have leſs knowledge, in Nature's healing powers, than the gentleman to whom this boy fortunately applied for relief.

I have known an accident, in moſt reſpects ſimilar to the above, where a child's finger, which had been fractured, was evidently loſt by the application of a poultice immediately to the ſore, which prevented it healing by *the firſt intention*, and was injurious alſo on account of its weight.

The introduction of the following caſe ſhews Dr. Hunter's advice to his pupils, to treat the *compound* in the ſame manner as the *ſimple* fracture, and I believe no one will deny that improvement to be his. If it ſhould be hereafter diſputed, the Doctor's numerous pupils can prove this, as well as they have done his right to many other important medical improvements and anatomical diſcoveries.

A curious Caſe of a Compound Fracture, related by Dr. HUNTER *at his Lectures.*

SPEAKING of the nature and cure of *ſimple* and *compound* fractures, Dr. Hunter obſerved, in his lectures, that, in treating the *compound*, many ſurgeons did miſchief, and irritated the wound, by their officious and artificial manner of dreſſing it. Inſtead of that practice, he recommended treating the *compound*, as much as poſſible, in the ſame way as the *ſimple* fracture: and in confirmation of that practice, uſed to relate the following ſingular caſe, which was always heard with great attention, becauſe the inſtruction was conveyed in the way of pleaſantry.

" A maniacal patient, Mr. G——, who was confined in the Infirmary at Edinburgh," (he ſays it was about thirty years ago) " ſeeming to have recovered a calm and rational ſtate of mind, was allowed to take an airing in the garden by himſelf. Here he took the reſolution of making his eſcape; and got over the garden wall. In dropping himſelf from the wall, which was very high, he pulled a large cape-ſtone along with him, and ſuffered a very bad *compound* fracture in his leg. He was carried round, and lodged again in the Infirmary, in this unhappy condition; and the ſurgeon, who was preſently brought to him, ſet the leg, dreſſed the wound, applied the eighteen-tailed bandage, &c. in the uſual way. After all this, the patient appearing to be very calm, the ſurgeon gave ſome proper directions, went away, and the patient was left alone to get ſome reſt, which was thought proper, and ſeemed to be his own deſire. His madneſs now took a ſingularly whimſical turn: he knew very well that he had got a miſerable broken leg; but his crazy imagination made him believe, that the ſurgeon had miſtaken the leg, had beſtowed all his cunning upon the ſound leg, which required no attention, and had left the ſhattered limb to ſhift for itſelf. Under this firm perſuaſion, convinced that his ſurgeon was too ignorant to perceive his blunder, too conceited to be ſet right, and too proud to ſuffer ſuch humiliation, he thought it would be moſt prudent, in his preſent ſtate of ſubjection, for the cure of his broken leg, to make the beſt uſe he could, of the judgment and dexterity which God had given him. He removed the whole *apparatus* from the broken leg, with great attention, that he might be able to apply it to the other leg, ſo exactly in the ſame manner, that the ſurgeon ſhould not be able to diſcover the alteration; and, leſt any ſuſpicion ſhould ariſe, and lead to an inquiry and diſcovery, he thought he ſhould be ſtill more ſecure, by ſecreting or hiding the other leg, that it might not be found, and appear in evidence againſt him. He therefore tore a large hole in the ſheet and featherbed, and buried the wounded leg among the feathers.

Next day, when the ſurgeon viſited him, he ſaid, that for a while he had been in pain, but that by a fortunate and accidental motion of the foot, the pain went off, as by a charm; that he had continued perfectly eaſy ever ſince; and therefore was reſolved to keep it as ſteadily as poſſible in the ſame ſituation. The ſurgeon finding him eaſy, the pulſe quiet, and no ſymptom whatever of fever, went to the foot of the bed, and lifting up the clothes, ſaid, Let us juſt ſee how the foot and leg look. The patient ſeemed much alarmed with the propoſal, and entreated him, for mercy's ſake, to deſiſt; becauſe, he ſaid, the leaſt motion in the world would diſturb it, and bring all his pains back again. The ſurgeon aſſured him that the bed-clothes touched nothing but the cradle, and that the lifting of them up could not in the leaſt move either the leg or foot; and then, obſerving to the ſtudents that the appearance of the foot was as favourable as he could wiſh, he expreſſed his ſatisfaction, and went away. Every day's viſit, after this, turned out equally ſatisfactory, both to the ſurgeon and patient, till the fifth or ſixth day, when the ſurgeon grew very anxious to ſee the wound, leſt any lurking miſchief ſhould be concealed, and was determined to remove the dreſſings. This the patient reſiſted, firſt with prayers, and then with imprecations and rage; but at laſt he was obliged to ſubmit. The ſurgeon, with a cautious and tender hand, removed the bandages, and, as he went on, expreſſed the pleaſure which he felt on ſeeing the ſkin, both above and below the wound, in ſo natural a condition. At length he lifted up the dreſſings, which he found were quite looſe, and, ſeeing a leg now perfectly ſound, which, a few days before, he had ſeen in ſuch a lamentable ſtate, you can better conceive than I can tell how he looked. After a ſhort pauſe, he paſſed his fingers along the *tibia*, and then ſaid, I only know that a fracture and wound there certainly was, and now there is certainly neither. Preſently he recovered himſelf enough to recollect that it was the other leg which he had ſet and dreſſed; and ſaid, Where is the other leg? turning off the bed-clothes at the ſame time. Lunaticks are quick in reſources, not eaſily put out of countenance, and imagine that no body can doubt what they aſſert. Mr. G——, ſenſible now that the leg would be diſcovered, drew it out from among the feathers, ſaying, with great expreſſion of reſentment and rage, that he would now expoſe the ſurgeon's ignorance to the whole world; that he always knew ſurgeons to be a ſet of ignorant fellows, though they wore large wigs; and now he would prove it, by a ſhocking inſtance, to the ſatisfaction of all preſent. This leg, ſaid he, holding out the broken leg, with a great cake of blood and feathers cruſted over and round the wound, this leg, thank God! is as ſound as any man's:—there, pointing to the other, is the broken leg,—you ſee what a diſperate condition it is in;—and that fellow, being called, did nothing for it:—he was called to ſet a broken leg; but he did not know a broken leg, and bound up this. After venting ſome more of his indignation and rage in ſarcaſtic and coarſe language, he begged that ſome of the young ſurgeons would bind up his broken leg again (meaning the ſound one) for that it was in great pain, was much diſturbed with this impertinent examination, and, if not taken care of, would make him a miſerable object, at beſt a cripple for life. The ſurgeon ſeeing his patient's imagination ſo ſtrongly perverted, and being convinced by the agitation which that miſapprehenſion had raiſed, that it would be, upon the whole, ſafer to indulge him in his wild conceit, with humanity as well as good ſenſe, deſired the young men to humour him, by putting the *apparatus* on the ſound leg. From that time he was calm, and, in all other things, reaſonable. The cure went on with perfect ſucceſs;—the ſcab of feathers at laſt dropped off;—the wound was then found to be healed, and the callus compleated: A memorable leſſon for ſurgeons, and a ſtriking inſtance of the weakneſs of human reaſon, of the imperfection of our boaſted art, and of the power of Nature!"

On RUPTURES.

A Rupture is a diſeaſe much more common than is generally imagined; when the inteſtine is ſtrangled, and cannot be returned, an operation of a very painful and dangerous nature muſt be performed: it were to be wiſhed that this operation might be rendered leſs frequently neceſſary. The *clyſma fumoſum*, or tobacco clyſter, is employed in the cure of *ſtrangulated herniæ*; and the operation may often be prevented by a timely recourſe to this excellent remedy. Some ſurgeons prefer the infuſion of tobacco to the fumes, which are to be injected glyſter-wiſe, and both are occaſionally ſucceſsful. Mr. Pott, in his *Treatiſe on Ruptures*, recommends the infuſion to be made by pouring one pint of boiling water on one drachm of tobacco.

The particular machines for this purpoſe (either bellows and a worm tube, or a large ſyringe) convey a great quantity of the ſmoke of this powerfully ſtimulating plant into the inflamed but torpid inteſtines, frequently procuring a copious evacuation of their indurated contents, and a return of the hernia, when all other means had been tried without ſucceſs.

Mr. Pott, in his Chirurgical Obſervations, p. 117, recommends the application of cold diſcutients, rather than warm relaxants, ſuch as poultices, which have no effect beyond the ſkin, and therefore cannot remove the ſtricture made by the tendon. Of the firſt claſs are ſolutions of *ſal. ammoniac crud.* in vinegar, the *ſp. mindereri*, *acet. lythargirit.* and ſuch like. He likewiſe cautions practitioners againſt handling herniæ too much in order to procure a return of them, as ſuch attempts are hazardous when the inteſtine is highly inflamed, and may probably bring on a gangrene.

Mr. Sharp writes thus: “I believe I may venture to ſay, that cold water, applied to this ſpecies of inflammation, has a dangerous tendency.”—See his Critical Enquiry, page 19.

It behoves every patient who has a returnable rupture, immediately to have a proper truſs, to prevent a deſcent of the parts; and it ſhould be conſtantly worn; ſince, without this ſecurity, he would be in hourly danger of his life. —This caution is of ſuch importance, that it cannot be too often repeated. The greateſt care muſt likewiſe be taken that the gut does not ſlip down under the truſs, the preſſure of which would give great pain, and might produce the worſt of conſequences: this muſt be particularly noticed by the parents of children; grown people would be directed by their own feelings to remove ſo great an evil. Youth may expect a perfect and radical cure of ruptures from an early application to bandage, which often produces an union and conſolidation of the apertures or rings of the abdomen. In advanced life bandages are generally palliative, yet a ſufficient preſervative from any fatal miſchief. All internal remedies in theſe caſes, except ſuch as prevent coſtiveneſs, are idle and trifling, or meant to impoſe, and are adminiſtered only by empirics; when the contents of a rupture are not returnable, and at the ſame time not ſtrangulated, a ſuſpenſory, or truſs, affords conſiderable relief.

A New Method of introducing Mercury into the Circulation, for the Cure of the Lues Venerea.

IT ſeems univerſally admitted that the *lues venerea* enters the blood, and circulates with other fluids for ſome time before it affects the ſolids: the removal of it is therefore to be effected by the introduction of mercury into the circulation, in conſequence of which this virus is expelled by the ſecretory and excretory, the ſalivary, urinary, cutaneous, or inteſtinal glands, and in this way the venereal taint in the ſolids themſelves is deſtroyed.

I am induced from obſervation to think this diſeaſe is in a fair way of cure when there is a ſalivary diſcharge occaſioned by mercury, ſince when theſe glands are affected, we have a convincing proof of the mercury's having entered the blood in a conſiderable quantity, or at leaſt that it has powerfully affected the glands. At this critical juncture the venereal ſymptoms, whether tumors, eruptions, or pains, begin to abate, till at length the cure is accompliſhed.

The firſt object therefore is to procure the admiſſion of mercurials into the blood veſſels. For this purpoſe, ſome prefer internal medicines; and many, external mercurial frictions in the form of unguent.

Internal mercurial medicines are uſually given in pill or bolus. As ſome of my patients had expreſſed ſtrong averſion to both, I was induced to give them the *merc. dulc. (calomel)* in a few drops of ſyrup, and was ſurpriſed to find their mouths much ſooner affected by this than by any other way :—a fact I further aſcertained by repeated experiments on myſelf.

The medicine was the ſame I gave others in pills. My dependance at that time was on the ſubſtance, not the form. I was ſurpriſed there ſhould have been ſo great and continued attachment to a form which few can endure, though the pills are ever ſo ſmall? It will ſcarce be denied, that there is a ſtrong probability of not mixing the ingredients ſo accurately that there ſhall not be ſome difference, as alſo in dividing them afterwards, eſpecially if the maſs be large; add to this the uncertainty of their diſſolution in the ſtomach.

Mercurial preparations, unleſs guarded with opium, vellicate and injure the ſtomach and inteſtines, organs moſt eſſential to health. Then why ſhould we not make trial of another method, not unreaſonable in ſpeculation, and reducible to practice? There have been ſo many inſtances where the mouth has been made ſore from the application of precipitate, or other mercurial powders, to ulcers, (parts not covered by the external ſkin), that every ſurgeon will readily admit the poſſibility of the abſorption of mercurial powders. May not mercurial powder alſo be abſorbed when applied to a part of the body where there is no outward ſkin? My experience tells me, it may; I have known the uſual effects of mercury on the mouth produced by my patients rubbing with their finger, moiſtened with ſaliva, three or four grains of calomel every day on the inſide of the cheek, on and around that place where the parotid (ſalivary) duct opens into the mouth, the diſeaſes were accordingly cured.

Since the publication of the firſt edition, in order to prevent the inconvenience that may ariſe from ſwallowing any of the powder, I have directed my patients for ſome months paſt to apply the calomel principally to the lips, parts particularly well calculated to bear friction, as are alſo the external ſurface of the gums; both being anterior to the inſide of the mouth, little or none of the mercury can be ſwallowed, but the whole will be abſorbed in the firſt inſtance. Should practitioners ſuſpect the mere ſurface of the lips are too ſmall to admit the abſorption of a ſufficient quantity, the inſide of the mouth may be alſo employed.

I did not formerly relate any particular caſes of my ſucceſs, nor was I aware that it would be neceſſary; ſome of my friends have lately intimated that my practice would be ſtill clearer if I gave ſome caſes by way of illuſtration. If I had had this originally in view, my caſes would have been more accurate, and ſtill ſtronger confirmation of my doctrine; but though there were a great many inſtances of my ſucceſs, yet, as I did not recollect every particular, and had not taken notes at the time, I durſt not charge my memory with the circumſtances.

The preſent caſes are what have happened to me very lately, and in which it was poſſible that I could recollect all the circumſtances, as they were ſtill recent in my mind.

CASE the FIRST.

A. B. came to me with a large chancre upon the præpuce, which had been there almoſt two months; he had alſo a bubo in each groin, one of which was in a ſtate of ſuppuration, the other indurated. I gave him the calomel, three grains every night, and two in the morning; it neither griped nor purged; the third day his mouth became ſore; in four or five days after, employing the mercurial friction, the ſore put on a fine florid aſpect, and had a white edge; the buboes began rapidly to diſperſe. I continued the calomel in ſmaller doſes, ſo as to keep up the tenderneſs of the mouth for three weeks, by which time the chancre was compleatly healed. I ſhould now have deſiſted from the mercury, being perfectly ſatisfied with what I had done; but Dr. Hunter, who was conſulted upon this caſe, adviſed the continuing mercury a fortnight longer, leſt he ſhould relapſe. I continued it at intervals for this period. He has remained well, though now ſix months ago. Mr. Maxwell the apothecary alſo frequently ſaw this patient while he was under cure, and knows the circumſtances to be as they are now related.

CASE the SECOND.

C. D. came to me with a ſore upon his forehead, about the ſize of a ſixpence, with thick callous edges. He had chancres about a month before, and had given over the uſe of mercury too ſoon. I had no doubt of its being venereal, and gave him the calomel;

he took two grains twice a day, rubbed only upon the lips; this he continued to do for three weeks; the ſore was quite healed in that time. I continued the mercury, two grains every day, for a fortnight longer: he had glandular ſwellings of the neck, which alſo, during this proceſs, went away. Mr. Cruikſhank frequently ſaw this patient during his cure, and heard him declare he was neither griped nor purged by the proceſs.

CASE the THIRD.

E. F. had a hoarſeneſs and pain in her bones, the remains of a lues ſuppoſed to be cured two years before, but had alſo chancres of a recent date. She took three grains of calomel in my method twice a day. Upon the third day ſhe began to ſpit gently; had no griping or purging: a profuſe ſweat came on at this time, and the hoarſeneſs and pain in her bones almoſt immediately left her. The ſalivation continued for a fortnight or more without any addition of mercury, and the chancres were cured. She appeared perfectly well, and I thought I had done enough, as I am by no means convinced that much mercury, but the method of employing it, is the chief circumſtance in curing this diſeaſe. About three weeks after, ſhe returned and appeared to be in a very poor way; ſhe now informed me of a complaint ſhe had formerly ſuppoſed to be the piles, but which I found was a venereal ficus; had I known of this I would not have interrupted the mercurial proceſs. I was now obliged to give her the calomel again in the ſame way, which again made her mouth ſore, and had the ſame effects as before: as ſhe got well I continued the calomel in ſmaller doſes for ſome little time; ſhe was cured in a few weeks, and has continued ſo ever ſince, though it is near five months ago. Dr. Hunter alſo ſaw this patient while under cure, and after every diſagreeable ſymptom was gone.

CASE the FOURTH.

G. H. about thirty years of age, had a ſmall chancre upon the præpuce, he had alſo a gonorrhœa and uneaſy ſenſations in the inguinal glands. I gave him two grains of the calomel twice a day, which were rubbed upon the ſurface of the lips only; upon the fourth and fifth day his breath was become fœtid, and his mouth tender, and by the ſeventh day the chancre was entirely healed. There was not the leaſt remains of uneaſineſs in the inguinal glands, and the gonorrhœa was much better, though not then perfectly removed.

CASE the FIFTH.

I. K. had chancres upon the glands of the penis, with phimoſis. As the chancres could not eaſily be dreſſed, they had made a very rapid progreſs. I thought it neceſſary to affect the mouth as ſoon as poſſible, and to throw in a very large quantity of the calomel; accordingly the firſt day he took three grains twice a day, but the two next days the mercury was increaſed to ſix grains twice a day. The ſecond day he felt his mouth tender and inflamed, upon the third day the ſalivation came on, and continued pretty ſmart for a fortnight; the progreſs of the diſeaſe was checked the inſtant his mouth became ſore; the ſymptomatic fever left him, and he gradually got well, and at the end of a month was compleatly cured. Mr. Savage (apothecary) ſaw this patient frequently during his illneſs.

CASE the SIXTH.

L. M. applied to me in March laſt, with a depaſcent (ſpreading) venereal ulcer in the integuments of the abdomen below the navel, near the ſize of a ſhilling in circumference, in depth the ſore would have contained two kidney beans. This patient ſuffered extreme pain for ſeveral days before ſhe came to me. I directed her to rub two grains of calomel on the inſide of the mouth twice a day for three or four days, at the expiration of which time ſhe complained of her mouth being very tender and painful, and her breath affected. The ſore immediately became eaſy, wore a kind aſpect, granulated well, and was entirely healed within a month. I continued the calomel ſome time longer, in ſmaller doſes, to prevent a relapſe: ſhe has remained perfectly well ever ſince. The wound was dreſſed with oil and a ſoft cerate. She was not purged with the medicine, looked healthy and florid in her countenance, and had remarkable good ſpirits while under cure. She was not confined a day at home on this account, and came to me twice or thrice every week.

I would not have the reader ſuppoſe that I always mean to raiſe ſalivation, or that the venereal virus paſſes off more ſpeedily from the increaſed flow of ſaliva; but the quantity of mercury which I judge neceſſary to ſtop the progreſs of the diſeaſe, and to facilitate the cure in many conſtitutions, will have this effect. The ſalivation however is ſo gentle, that they have ſeldom been put to much inconvenience, never confined to the houſe, and it was hardly to be diſcovered when they were careful to conceal it. Many of my patients took the mercury in very large quantities without being ſalivated at all, and were equally well cured. I think it better however, upon the whole, to run the riſque of a ſlight ptyaliſm, than not to throw in a ſufficient quantity of the mercury. I muſt alſo add here, that in no one inſtance that I raiſed a ſalivation were there ulcers on the inſide of the mouth, as is common in other ſalivations. One or two grains applied behind the preputium, or to the labia, will ſometimes of themſelves give a mercurial breath and ſlight ptyaliſm, and, joined to the other, will therefore greatly advance the cure. This may be daily uſed, moiſtened with ſaliva. It is probable the latter method will cure the gonorrhœa alſo. It may even act as a prophylactic (or a preſervative.)

Let us now compare the two ſorts of frictions.—The internal friction on the cheek requires only a ſmall ſurface and ſlight exertion. It is neat, commodious, may be ſoon performed, and is ſure, if continued, to affect the mouth and other ſecretions.—-The external friction with unguent is exceedingly diſagreeable, generally inflames the ſkin, and occaſions delay by a temporary diſcontinuance on that account. Sometimes it will affect the mouth, ſometimes it will not; nor indeed any other of the ſecretions; for which reaſon no certain dependence can be placed on it.

The experience I have had of this new mode has ſatisfied my mind that it is practicable. The conviction I had of the excellence of this method was indeed ſo great, that I ſhould have depended ſolely upon it, but from the viſible deviation from the uſual method; which circumſtance might have drawn forth complaints of trying experiments, and rendered me obnoxious to cenſure. I ſhould be glad the profeſſion (who certainly are the only people duly qualified) would proſecute theſe enquiries further, and I truſt they will not be diſappointed in the reſult of them. I have been ſomewhat diſcouraged in publiſhing theſe remarks on this new method of employing mercurials, by ſome of my chirurgical friends, and it may be a juſtice due to them, to ſay ſo; yet being ſtrongly perſuaded of the truth of what I have advanced, I knew not how to forbear communicating my obſervations to the public; this mode of practice being at leaſt as ſafe as any other, infinitely more expeditious in my opinion, which, I hope, will be conſidered as a ſufficient apology for the publication. All the other methods at times ſtand in need of aſſiſtance: this, I am ſure, will at leaſt be a uſeful auxiliary; as ſuch let it be tried at firſt. When this method ſhall be introduced into general uſe, which I truſt it will when its ſuperiority ſhall be fully aſcertained, it will be found to cure every ſtage of this diſeaſe in the moſt eaſy, expeditious, and effectual manner.

I am aware it may be ſaid, the mercurial powder paſſes into the ſtomach, and effects the cure in the uſual way. Even if this was the caſe, the patient would more certainly be cured than if he took this medicine in pills, ſince theſe may paſs through the body undiſſolved; but thoſe who uſe the powder in the manner I recommend, refrain from ſwallowing the ſaliva till the abſorption is effected. A low regimen, however neceſſary in the gonorrhœa, ill accords either with the lues or its antidote mercury: cordial medicines and a nouriſhing diet are requiſite, and will increaſe the beneficial effects of this remedy.

Having obſerved that mercurial preparations irritate or vellicate the coats of the ſtomach and inteſtines, when given in pills, (a form diſagreeable to many patients), it will be ſaid the powder muſt produce the ſame effect if it is ſwallowed. To this I anſwer, the powder incorporates with the ſaliva, which corrects the ſtimulating quality; and ſix or eight grains of this preparation will often produce leſs purgative effect than two grains in a pill.—A fortunate circumſtance ſurely for many patients.

I ſhall here inſert an account of the true venereal chancre, from the learned Dr. Aſtruc, to ſhew the ſort of ulcer which I have cured, (and that it is not a ſimple excoriation from any other

cauſe),

cauſe), by this new method of adminiſtering a mercurial antivenereal remedy of long eſtabliſhed reputation, and of undoubted efficacy, if fairly admitted into the circulation: "At firſt there "ariſes a ſmall miliary, red, pointed, "hot, itching pimple, the top of which, "by degrees, grows whiter, and more "even, and at laſt opens with a ſmall "aperture, from whence a ſmall quan-"tity of ichor is diſcharged. The ero-"ſion of the lips continuing, the ulcer "increaſes, is dilated, and forms a ſmall "cavity; it is ſurrounded with callo-"ſities more or leſs hard, and thick; "it abounds with a thick, viſcid, tena-"cious pus, and frequently produces "ulcers of the ſame kind in the neigh-"bouring parts."

We muſt not believe that this complaint is cured when the ſore is entirely healed, if there is any hardneſs left in the ſkin, there is ſtill room for ſuſpecting a relapſe; therefore the remedy muſt be continued till that dangerous ſymptom is ſubdued; elſe, the virus being in the blood, the almoſt extinguiſhed diſeaſe will rekindle, and terminate in a confirmed lues.

Mercurial fumigations to venereal ulcers have been always a common practice; and their ſubtle fumes, imbibed into the orifices of the veſſels, and received into the circulation, have often been of great ſervice in theſe caſes; and it is true likewiſe, that they have as often failed, and are now ſeldom uſed. I mention this method in particular as one that approaches to that of mercurial abſorption in the manner I have recommended; which, I truſt, on a candid and full inveſtigation of the facts, will appear to be attended with better ſucceſs than fumigations.

I do not wiſh it to be underſtood that the mercurial friction muſt affect the ſalivary ſecretion always; for I am well convinced it operates occaſionally on all or any of the other glands. I have known it, in particular, act powerfully as a diaphoretic, and alſo as a diuretic, immediately after it was applied to the inſide of the mouth; and my patient expreſſed his ſurpriſe at this effect, becauſe he had drank nothing for ſome hours. This brought to my mind what Dr. Hunter has aſſerted in his Lecture on the Circulation of the Blood.

"One proof that the blood circu-
"lates," he ſays, "is, make an orifice,
"and inject a fluid of a purgative qua-
"lity, a diuretic, or any other, and it
"ſhall affect the particular gland ac-
"cordingly; that is, purge, vomit,
"ſalivate, go off by urine, &c. accord-
"ing to its nature."

It is well known the *abſorption* of ſuch medicines will produce the ſame effect as the *infuſion* of them into the blood. The ſmall globules in the *unguent. merc. cærul.* are abſorbed by the pores of the *external* ſkin, and aqueous particles are imbibed by rubbing the face and throat with a wet towel, which notoriouſly allays thirſt, the linen becoming ſoon dry from the abſorption produced by the friction.—*Internal* abſorption is ſatisfactorily demonſtrated by the nouriſhment afforded to the ſick by *egg* and other *glyſters,* when they are deprived, by any accident, of the power of deglutition; a remarkable inſtance of which fell under my own obſervation in St. Bartholomew's hoſpital. A poor woman, in a phrenzy, cut her throat. She divided both the trachea arteria and the œſophagus; and therefore the food, in its deſcent towards the ſtomach, would have endangered ſuffocation. She was the late Mr. Nourſe's patient. He ſewed up and dreſſed the external wound, and ordered glyſters of eggs, &c. to be injected *pro re natâ.* The woman lived a fortnight by this ſuſtenance alone, (the aliment being received into the lacteals; the abſorbent veſſels of the inteſtines;) but, reſolved on her own deſtruction, ſhe at length accompliſhed it by violence on the wound, notwithſtanding the utmoſt care and diligence were uſed to prevent her.

That I may not be thought to have haſtily or inconſiderately publiſhed this doctrine of *mercurial abſorption* from the mouth, it becomes neceſſary for me to ſay, it is more than five years ſince the experiments and obſervations I then committed to writing were communicated to my friends; and that occaſionally I have continued to make uſe of this mode with the deſired ſucceſs. Beſides, (ſince the other parts of my Eſſay went to preſs) I have been honoured by Dr. Hunter and Mr. Cruikſhank with ſeveral converſations on this ſubject. I am happy their ideas coincide with my own, and that I have an opportunity of rendering an eſſential ſervice to mankind by the publication of their opinions.

Dr. HUNTER'*s Remarks on Mr.* Clare'*s New Method of introducing Mercur into the Circulation.*

"AS the *external* ſurface of the body (ſays the Doctor) is every where bibulous, ſo is the *internal.* There can be no doubt of abſorption taking place on the inſide of the mouth, of the *preputium,* of the *labia,* (lips), &c. and any fine powder, capable of being abſorbed there, will, no doubt, be more readily abſorbed when mixed with a watery fluid. When mixed with oil, and applied to wet ſurfaces, it may be preſumed that it will be kept at ſome diſtance. It is likewiſe very probable, that, when the calomel is rubbed upon the ſurfaces of the mouth, what gets into the ſtomach is carried down ſo gradually, and diffuſed over ſo great a ſurface, that it will ſtimulate the *primæ viæ* much leſs; and people with very tender bowels may therefore bear it much better than when given in pills, boluſes, &c. And it is reaſonable to ſuppoſe, that watery fluids will, in general, be more readily abſorbed, perhaps even when applied to the outward ſurface."

Mr. Cruikſhank's opinion will be perfectly underſtood in the following letter, and, ſo far as the abſorption of calomel is concerned, appears to be nearly the ſame with Dr. Hunter's.

The uncommon pains taken by Mr. Cruikſhank, in examining and recommending this new ſyſtem, demand my warmeſt acknowledgements. I confeſs myſelf infinitely obliged to him on this, and other occaſions; and I wiſh he may ever meet with the ſame firm and diſintereſted friendſhip from others, which he has ſhewn me, and which his great abilities give him a juſt title to receive from all lovers of ſcience and patrons of merit.

Fig. 2.

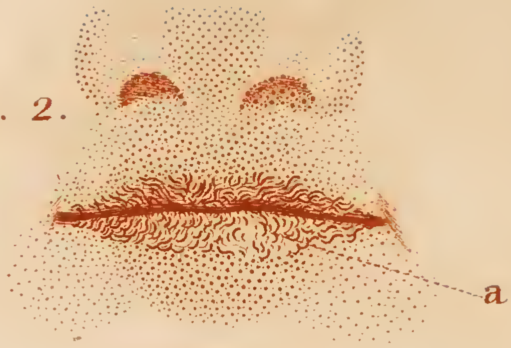


Fig. 1.

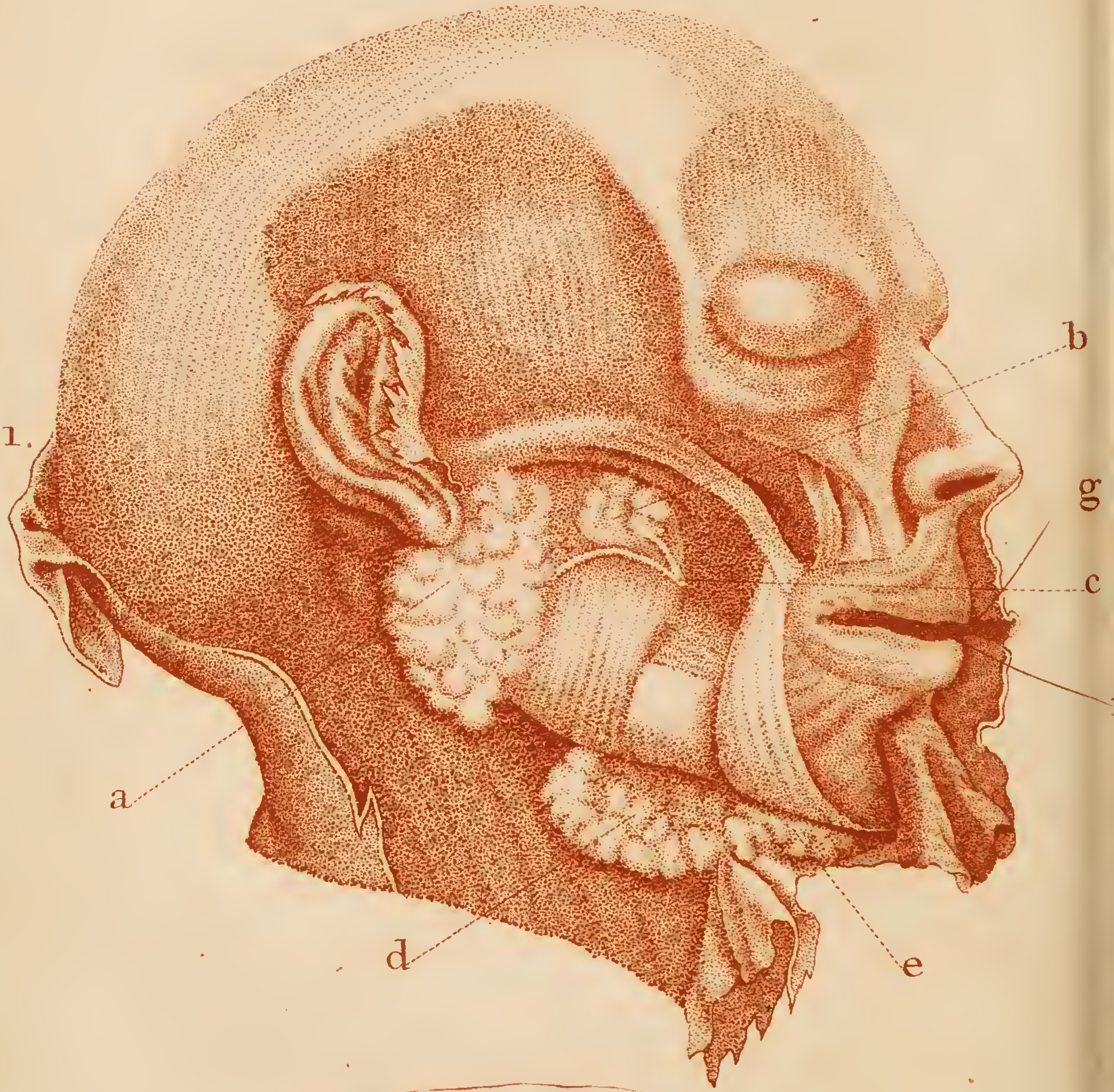
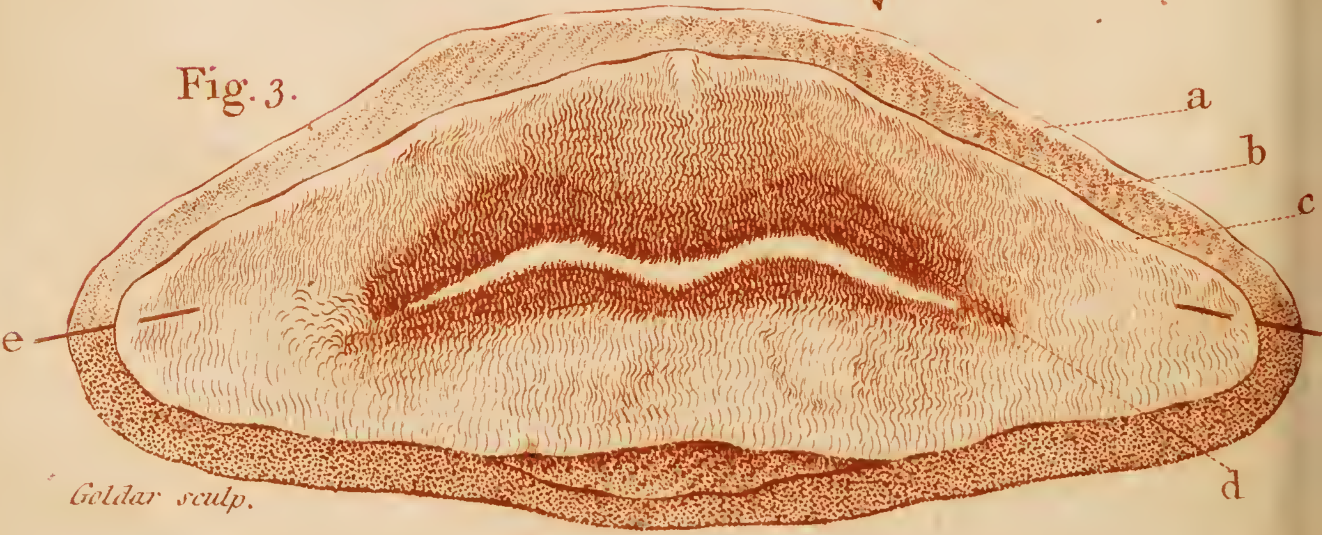


Fig. 3.



Goldar sculp.

Explanation of the PLATE.

Fig. 1. Is meant to ſhew the ſalivary glands, for which purpoſe the integuments are removed from the head and face.

a The parotid gland.

b A not uncommon ſtraggling portion of the ſame gland, with its own little duct, which is ſeen joining,

c The parotid duct; this laſt opens upon the inſide of the mouth, as at *e* in Fig. 3.

d The maxillary gland.

e The ſublingual gland, drawn a little down to bring it into view.

f A briſtle, ſuppoſed to be in the parotid duct, ſhewing its direction.

g A ditto in the duct of the maxillary gland ſhewing ditto.

Fig. 2. Shews the outſide of the lips ſtripped of its cuticular covering.

a The villi, (or packets of elongated blood veſſels, abſorbents and nerves), here exceedingly long.

Fig. 3. Exhibits ſo much of the inſide of the mouth as is upon the outſide of the teeth and alveolar proceſſes.

a The cut edge of the cutis, or true ſkin.

b The fat, or adipoſe membrane.

c The villous membrane, or lining of the mouth, ſtripped of its cuticular covering.

d The villi, as at *a* Fig. 2. but ſhorter.

e e Briſtles in the ducts of the parotids, at their opening into the mouth.

FINIS.

REMARKS

ON THE

ABSORPTION of CALOMEL

FROM THE

Internal Surface of the Mouth;

Accompanied with

A Preliminary Sketch of the Hiſtory and
Principal Doctrines of Abſorption in
Human Bodies.

In a LETTER to Mr. CLARE,

BY

WILLIAM CRUIKSHANK, A.M.
Reader in Anatomy.

LONDON. MDCCLXXIX.

SIR,

WHEN you ſhewed me your Eſſay *on the Cure of Abſceſſes by Cauſtic*, and on your *Method of introducing Mercury into the Circulation*, you were ſo obliging as to deſire my opinion on the latter of theſe ſubjects. I gave you my opinion; and your Eſſay has now run through a firſt edition.

That quickſilver, divided by the intervention of hogs-lard, (and forming mercurial ointment), could be introduced into the circulation, from the ſurface of the ſkin, or that almoſt all of the preparations of mercury might be introduced into the circulation, from the internal ſurface of the inteſtines, was very well known; but that mercury, in the form of a powder, could find its way into the blood, from the internal ſurface of the mouth, in ſufficient quantity, to cure the venereal diſeaſe, unconnected with any inteſtinal abſorption, was a new doctrine!

I did not expect you would be believed; the doctrine of abſorption is ſtill in its infancy; nor have I found that either you, or I, have had ſufficient influence to induce belief. The doctrine notwithſtanding may be true.

It would be wrong in you to complain of your fate; mankind will not receive new doctrines at firſt hearing; nor have you any right to expect they ſhould. Inoculation itſelf met with the moſt furious and unjuſt oppoſition.

My opinion of your method, in the former edition, was ſtiled *a long Letter*; I have not now made it ſhorter. Whenever there is the leaſt probability that my opinion may be of uſe to gentlemen of the profeſſion, or indeed to any ſerious man, I hope I ſhall never appear *reſerved* or *unneceſſarily cautious* in giving it.

The abſorbent ſyſtem has, for ſeveral years, *particularly* engaged my attention; nothing, in the ſmalleſt degree connected with it, could poſſibly meet with *indifference* from me.

Common civility entitled you, as a gentleman, to any opinion I could form on your ſubject; but when I reflected, that you had once been a pupil of Dr. Hunter's, my preſent ſituation with him made me feel the *ſtrongeſt inclination* to do any thing in my power to oblige *his former* as well as *our preſent pupils*.

When I firſt ſent you my opinion, in writing, concerning the abſorption of calomel from the mouth, my remarks were leſs extenſive than thoſe which afterwards appeared in print; for when I underſtood, that you wiſhed my opinion might be made public, I felt that my duty to the public required me to be ſomewhat more explicit.

Some time has now elapſed ſince this letter was firſt offered to the world. I have been cenſured by ſome for having too warmly attached myſelf to one who appears to have been, at that time, an entire ſtranger to me; others have not ſcrupled to ſay, that I had too haſtily recommended your practice.

That you had been diſcouraged by your medical friends in your important reſearch; that you yourſelf appeared diffident in talking of what, you notwithſtanding, believed, you had done, added to the reaſons already mentioned; muſt plead my excuſe (if any can be wanted) for my warmth of attachment.

As to the ſecond accuſation, I have only to ſay, that it is not abſolutely impoſſible that we ſhould both be miſtaken. I do not yet feel, however, that I have any reaſon to retract my opinion, or the leaſt cauſe to repent of my having endeavoured to ſupport the abſorption of calomel from the mouth. I ſeriouſly declare, that in a matter of ſo much moment, in which the health of millions *might be* concerned, and where my own reputation *was actually* at ſtake, no motive could have induced me to recommend a practice I ſecretly ſuſpected would not be attended with ſucceſs.

The great hinge on which your method turns, is, the abſorbing property of the human body. You affirm that your calomel is abſorbed by the lymphatic veſſels of the mouth. I find no difficulty in believing that it is; and am ſatisfied that this, as well as many other powders, may be abſorbed by the ſurfaces of the body.

Before I ſtate the arguments which preſent themſelves to me in favour of this abſorption, I ſhall take the liberty of making *ſome preliminary remarks on the abſorption of the human body in general*. I have ſeveral reaſons for doing ſo.

In the former edition of this Letter, the object of which was the abſorption of calomel, I had already, *unavoidably*, thrown out many remarks on this important ſubject. I did not, on reviſing theſe, find that I had changed my opinions; but as I was at that time much hurried, from my public employment, I could not help wiſhing that the Letter might be ſtill *more correct*; and accordingly I have now ventured to withdraw thoſe ſcattered remarks, interſperſed with the arguments for the abſorption of calomel, from the place where they ſtood, and have thrown them together in the form of an introductory ſketch of the doctrine of abſorption, after which I return to the abſorption of mercury. Many of my anatomical friends had wiſhed to ſee ſuch a ſketch of the principal opinions at preſent held on abſorption

tion at *Windmill-ſtreet*: I ſaw no ſolid objection either to complying with their requeſt, or to embracing the preſent opportunity for this purpoſe.

Such a *ſketch* will appear ſtill leſs unſeaſonable or unneceſſary, if we reflect, that practical books in medicine are now much read by thoſe who, though not educated to medicine, wiſh to know *ſomething* of their own bodies: when they have been made a little acquainted with *abſorption in general,* they will be better able to judge of *your* abſorption of calomel.

Every body knows what is meant, when a dry ſponge is ſaid to abſorb water; it drinks up a certain quantity, and can take up no more till it becomes drier again. The index of the hygrometer (or its ſpring) abſorb moiſture from the atmoſphere, and occaſionally part with it again. In conſequence of this, the index points higher or lower in the ſcale, in proportion to the humidity abſorbed or given off. Fluids themſelves abſorb. Vitriolic acid, expoſed in a flat diſh to the atmoſphere, abſorbs from it, in the ſpace of a year, ſix times its own weight of water. All the ſurfaces and cells of the human body, and moſt probably of every living body, are alſo abſorbent, and take up, not only *fluids*, but the minutely divided particles of *ſolids* themſelves.

When ſponge, dried animal, or vegetable ſubſtance, or oil of vitriol, abſorb, it is in conſequence of properties belonging to *inanimate matter*, in conſequence of an attraction which ſubſiſts between them and the fluids they abſorb. The ſponge is porous; its pores, however, could neither prevent nor can accelerate the paſſage of the abſorbed fluid; in ſhort, it is perfectly paſſive. But the *abſorbent pores* of the human body are *the extremities of irritable veſſels*, which, in conſequence of a property dependent on *life*, *take up*, *or reject*, the preſented *fluids*, or *ſolids*, according to certain circumſtances.

In making my remarks on *abſorption in general*, I ſhall ſuppoſe myſelf, obliged to reply to ſuch queſtions as I conceive would naturally ſuggeſt themſelves to one, who having never heard of *animal abſorption* before, wanted to receive ſome information on this curious ſubject. His firſt queſtion, I preſume, would be,

I. *What proofs are there of this abſorption?*

THAT human bodies abſorb we have now many convincing examples.

Kaau Boerhaave appears to have wrote an expreſs treatiſe on the following paſſage of *Hippocrates*, " δηλον, ἥ αισθησις, " ως ἔκϖνοον, και εισπνοον, εϛιν, ὅλον τὸ " σῶμα" that is, *it is plain, to any one* *who attends, that the whole body is endowed with the properties of expiring and inſpiring.* *Hippocrates* does not here ſufficiently illuſtrate his meaning. The body inſpires when we breath the ſurrounding air; but this is not what we *now* mean by *abſorption.* It is probable, however, that he really means, as *Galen* (his beſt interpreter) and *Kaau Boerhaave* conceive he does, and that he is to be underſtood as ſaying, *the whole body perſpires and abſorbs.* We do not, however, want the authority, *even of Hippocrates*, to be ſatisfied of this circumſtance. *Boerhaave* quotes, in favour of human abſorption, the ſtory of *Democritus*, who was reported to have kept himſelf alive three days, on the *ſmell of new bread.* *Boerhaave* does not inform us how long *Democritus* had previouſly faſted before he begun to ſmell the bread. If he had not faſted at all before, it does not, in my opinion, prove abſorption; but rather, that a man may live three days without taſting any thing; which certainly may be believed. If he had not taſted any thing for three or four days before, it would look as if ſomething *nutritious*, and which was vapour in the bread, had been abſorbed. The Turks, it is true, are ſaid to travel fifty miles a day on a bit of opium held in the mouth; and feel ſtrong and well, merely in conſequence of that *excitement* which the opium produces in the brain. Mad men, whoſe brains are excited from ſome internal cauſe, can live long without food; but it would be inferring *too much* to conclude, that the mere *ſmell* of new bread could produce a ſimilar excitement in the brain, or ſimilar effects on the body.

Paracelſus ſays, That the antient *ſophi*, leaſt their intellectual powers ſhould be clouded by groſs feeding, and their contemplations diſturbed, never ſwallowed any food; that they were kept alive only by chewing it; held it in their mouths ſometime, till the finer parts were abſorbed, and then ſpit it out.

He alſo informs us, that he himſelf nouriſhed men for ſeveral days together, by applying nutritious liquors to the ſurface of the body. I cannot pretend to ſay what degree of credit is due to theſe aſſertions; nor reſpecting the proof for abſorption, does it much ſignify if they ſhould even turn out falſe.

We have been informed by very antient phyſicians, that old men have, in ſome degree, recovered their ſtrength, and lived longer, than they otherwiſe would have done, in conſequence of merely ſleeping with young women, and being in their company; that on the contrary, women ſo circumſtanced, have ſoon faded; have loſt their beauty, their plumpneſs, and ſtrength. It has hence been concluded, that the abſorbents of the ſkin in the older perſon, took up ſomething nutritious from that of the younger one. I have ſome doubts reſpecting the fact; I mean, I do not believe the one became really ſtronger, and the other weaker. If it could be proved however to be true, I ſhould rather be diſpoſed to account for it in another way. The preſence of a young woman may, by pleaſing the mind, or warming the imagination, ſtimulate the heart and arteries to greater action, and rouſe them from a torpidity already felt, or which they would otherwiſe ſoon fall into. *All Stimuli* are vulgarly thought *unnatural,* and productive of debility; but there are many *natural ſtimuli,* which actually ſtrengthen, inſtead of wearing out the body. The body would languiſh but for *exerciſe*; our *food* itſelf *ſtimulates*; the *moſt powerful medicines* are *ſtimulants*; and the body is generally more healthy when the mind is *much* and *greatly* occupied. On the other hand, the preſence of an old man, may commonly, have no pleaſant effect on the mind of a young woman, but the reverſe, and may prove a conſtant irritation (or teaſing) inſtead of a healthy ſtimulus; and her emaciation may be better accounted for this way. I ſhould alſo doubt, whether a young child ſleeping with an elderly nurſe loſt fleſh, from this circumſtance. However theſe ſuppoſed facts may be, we have enough of very convincing proofs of abſorption without them.

That our *Food*, which is converted into a milky fluid in the ſtomach and inteſtines, is abſorbed by the *lacteals*, and carried into the blood, *Aſelius*, above a hundred and twenty years ago diſcovered; and repeated diſſections of living animals have every day confirmed his diſcovery.

The furniſhing the body with freſh ſupplies in proportion to the waſte of blood, occaſioned by the *different ſecretions*, and the *forming* and *repairing* of parts may appear a ſufficient reaſon why ſome ſuch proceſs as abſorption, ſhould take place, on the inner ſurface of the inteſtinal tube. But abſorption may be ſuppoſed peculiar to this, and unneceſſary on other ſurfaces. Let us ſee if there are not ſufficiently ſtrong proofs, that the abſorbing power, alſo reſides in every other ſurface.

Some recent inſtances of the cure of *hydrocephalus* by mercurial friction, ſhew that water may be abſorbed from the ventricles of the brain.

There are ſome caſes of *hydrothorax* and of *empyema*, in authors of the beſt authority, where it appears, that water and purulent matter have been abſorbed from the cavity of the cheſt, and that the conſtitution has, in this way, cured itſelf.

After the operation for the *Empyema* has been performed, and honey and water, or wine, or bitter decoctions, have afterwards been employed as waſhes for the diſeaſed ſurface, there are *ſome inſtances* where the injected fluids have been taſted in the mouth.

Physicians have occaſionally ſeen caſes of dropſy, in which, after every medicine had been tried in vain, and the miſerable patient had been conſigned to his fate, the conſtitution itſelf has here alſo worked a cure, and the whole water of an *aſcites*, amounting perhaps to ſeveral gallons, has been abſorbed from the cavity of the abdomen in a few days, and carried off by ſtool or urine. Sometimes too, the fluid of *hydrocele*, has been abſorbed from the cavity of tunica vaginalis teſtis, in the dropſy of that part.

That fluids injected into the cheſt, have been taſted in the mouth; or water in the cavity of the abdomen, has paſſed off ſuddenly by urine or by ſtool; or that *Hydrocele* once formed has diſappeared; are no proofs, it may be ſaid, of *abſorption*. There may be ſome common paſſages between theſe parts and certain others, which we know are intended as the natural drains of the body; or if there are no ſuch paſſages as are here ſuppoſed, the *linings* of theſe cavities may allow fluids to tranſude. *Anatomy* demonſtrates there are no ſuch paſſages; if there were, what good reaſon could be given for a fluid's remaining ten months, or ten years, in theſe cavities, and never taking theſe paſſages before. It may be proved, that *tranſudation*, (*or the ſoaking of fluids through membranes*), does not take place in the living body, for if it did, it would be impoſſible that any man, who had water in his cheſt, ſhould not preſently have it in his abdomen; or having *aſcites*, ſhould not quickly have the whole cellular membrane of his body alſo loaded with the water. The liver, or gall bladder, has been found ſometimes *gorged* with bile, the moſt bitter of fluids, without the patient's having once during the diſeaſe, a bitter taſte in his mouth. The moſt fœtid pus of a *pſöas abſceſs* unopened, or the moſt putreſcent *alvine* ſecretions, while detained in the inteſtines, notwithſtanding the volatile vapour they appear to contain, never affect the olfactory organs of the body which contains them. If any fluid is more likely to pervade membranes than another, *oil* (or *fat*, which in the heat of the living body is liquid) might be ſuppoſed to be that fluid. It has indeed been ſuſpected of oozing through membranes, or the ſides of veſſels, and lubricating ſurfaces. The omentum, for example, has been ſuppoſed to ſerve the purpoſe of furniſhing oil, which tranſuding, lubricated the ſurfaces of the inteſtines, and made them glide more eaſily on one another in their periſtaltic motions. The truth, however, is, that oil, in the living body, *never tranſudes*; it is confined to certain ſpaces, and never paſſes its boundaries, which it ſurely would ſometimes do, if it was capable of tranſuding. There is no oil on the eye-lids, none on the penis, or any where within the ſcull. The fluid which lubricates ſurfaces, and gives them their eaſy play on one another, is manifeſtly *watery* and not *oily*.

Though the oil does not *tranſude*, it may however be *abſorbed* from its *cells*. It is accumulated in great quantity, in ſome habits, when in health, and may be a ſymptom that the ſyſtem is acting with vigour; or it may ariſe from a particular diſpoſition in the arteries, like that of *ſecreting earth*, and forming *calculi* in other habits; whatever is the reaſon of its being formed, the oil itſelf is conſtantly abſorbed when the body is deranged, and under any particular irritation, as in fever, dyſentery, dropſy, pulmonary conſumption, and many other diſeaſes. It is alſo abſorbed in the winter-ſleeping animals, who emerge from their lurking places in the ſpring, notwithſtanding their plumpneſs in the preceding autumn, almoſt free from every particle of fat. That the fat is abſorbed in the human body, under the diſeaſes mentioned, and when expoſed to hunger, has given room for ingenious conjectures, founded on the preceding fact, reſpecting the winter-ſleeping animals; and ſome phyſiologiſts have believed, that as in the one inſtance the fat was abſorbed, probably, to be converted into nouriſhment; ſo in the other it was abſorbed, and carried into the blood veſſels again, in order to nouriſh the body, either when from want, the ſtomach was not affording its ſupplies, or to nouriſh the body in a leſs irritating way, than that of throwing a quantity of food into the ſtomach, in an already diſeaſed ſtate of the machine, as well as of that organ. The jelly found in the ſtomach of fœtuſes, ſeems to ſupport this opinion. I have no idea that the fœtus drinks, or abſorbs the *Liquor amnii*, which, frequently at the end of geſtation, when the fœtus ſtands moſt in need of nouriſhment, is in exceeding ſmall quantity, and at other times in a very large and, ſeemingly, ſuperfluous quantity. It ſeems more probable that *ſomething* (probably the coagulable lymph) is abſorbed by the fœtus from the blood of the mother in the placenta;

this is afterwards ſecreted by the arteries of the ſtomach into its cavity, and converted partly into *chyle*, partly into *meconium*. Ingenious as the idea of abſorption of fat in diſeaſes, and in want of food, in order to its *converſion* into *nouriſhment* may be, and however probable it may appear in the winter-ſleeping animals, I have great doubts with reſpect to its truth in the human body. The body appears to me to be at theſe times too much under a powerful contrary ſtimulus, to be capable of the actions of health; ſuch as digeſtion, and nouriſhing the body. The ſtomach receives no food in the leaneſt perſon, under the irritation of an inflammatory fever, becauſe it can digeſt nothing. The only uſe of employing, in theſe caſes, liquids themſelves is, probably that they may be abſorbed without being digeſted; and that materials may be furniſhed for ſupplying the different ſecretions, which go on, and are ſometimes even increaſed notwithſtanding the fever. I have ventured to ſuppoſe that watery fluids thrown into the ſtomach are not digeſted, becauſe I am much diſpoſed to be of Mr. Hunter's opinion reſpecting digeſtion, who thinks that ſolids, or thoſe fluids, which in the ſtomach are capable of becoming ſolids, only, are properly digeſted, and capable of nouriſhing the body.

If it is urged, that, in order to nouriſh the body, the abſorbed fat may be only conveyed into the arteries, and by them be immediately applied as nouriſhment to parts, without ſuppoſing it neceſſary that it ſhould ever be ſecreted into the ſtomach, and digeſted; I have only to ſay, that it is not probable that any thing elſe than chyle can be converted into nouriſhment; and if the fat could be uſed for this purpoſe, the arteries are as much in a diſeaſed ſtate, under the preſent ſuppoſition, as the ſtomach, and equally, as I ſhould think, incapable of nouriſhing the body, as the other is of digeſtion, or forming chyle.

The mere ſtimulus of inflammatory fever, or the change which, like opium, it produces in the brain, probably keeps the body alive, and makes any nouriſhment unneceſſary. If fat was abſorbed in the human body for the purpoſe of converting it into food, it would moſt probably be abſorbed for that purpoſe in the healthy ſtate of the body, as in the natural and ſound ſtate of the winter-ſleeping animals; and if this was true, a fat man ſhould be able to bear hunger better than a lean man; and it would not be poſſible that a fat man ſhould die of hunger ſo long as he kept plump, which we all know is far from being the caſe.

Thus, abſorption takes place, not only in large cavities, but in the *cells* of the *adipoſe membrane* itſelf. It alſo takes place in *thoſe* of the *cellular ſubſtance*.

It has frequently happened that a *rib has been broke*, and its broken ends have entered the ſubſtance of the lungs; in conſequence of which, the air of inſpiration has eſcaped from the lungs, and got into the cellular membrane of the intercoſtal muſcles, and from thence has paſſed over the cellular membrane of the greateſt part of the body. The modern practice is, to let this air eſcape by punctures in different parts of the ſkin. This air has ſometimes, however, been left to itſelf, has diſſolved in the fluids of the body, as in common water, and been gradually abſorbed.

In the ſame manner, when a blood veſſel, from a fall or bruiſe, has burſt about the neck, and an *internal hæmorrhage* has taken place into the cellular membrane, the ſkin of the whole trunk of the body has appeared as black as that of an Ethiopian, from the quantity of extravaſated blood underneath; the blood, like the air, has been gradually abſorbed, and returned into its veſſels.

From obſervations I have made on thoſe who died of the *peritoneal inflammation*, I am convinced, that the abſorbents frequently take up the red blood from the *internal ſurfaces of the arteries themſelves*. In this inflammation I have ſeen parts of the inteſtines exceedingly inflamed in their peritoneal coat. When I came to examine this inflammation and conſequent redneſs, I found the blood, to which it was owing, was not extravaſated; but the greateſt part, at leaſt, was really in the cavities of the abſorbents, which, on the inteſtines, are eaſily diſtinguiſhed from the arteries and veins, and, on account of the greater abſorption intended to take place there, are proportionably larger than any where elſe. There is in reality little or no ſwelling of parts in this inflammation, of courſe little extravaſation into the cellular membrane; and, by preſſing the yet fluid blood forwards through the abſorbents, I could reſtore the reddened ſurface to its original white one. I believe, that *this abſorption, from the cavities of arteries,* takes place in conſequence of their being too much dilated; for diſtenſion of parts appears in many other inſtances to be one great cauſe of abſorption's taking place in them. When a gall ſtone ſticks in the *ductus communis choledochus,* and prevents the bile from paſſing into the inteſtines, the *porii biliarii,* or the excretory ducts, become diſtended; in conſequence of this, abſorption takes place from the cavities of theſe ducts, and *jaundice* is produced. If the milk is not ſucked from the nurſe's breaſt, the *tubuli lactiferi* become diſtended, and the milk is carried off by the abſorbents; and from its quantity, or its unuſual ſtimulus, diſtends and inflames the *axillary glands.* If the bladder is diſtended with urine, and an opportunity of diſcharging it cannot be had, it is quickly abſorbed, and returned into the blood; and the bladder, notwithſtanding its former diſtenſion, on attempting to make water ſometime after, is often found almoſt empty. When the natural cure of *aſcites* takes place, it is alſo, perhaps, in this way. I mean, that the abſorbent veſſels, which, for a long time before, appear to have loſt their irritability, to diſtenſion, from a change in the habit, (as ſudden as a bar of iron's loſing its magnetiſm from a ſtroke of a hammer), recover their irritability inſtantly, and abſorb in full vigour.

That abſorption takes place *on the ſurface of the body*, we cannot doubt.

Eſſential oils, rubbed on the feet, have, in ſome time after been taſted in the mouth; a proof, not only of their having been abſorbed by the veſſels of the ſkin, but that eſſential oils may paſs through the abſorbents, and even the ſecretory veſſels, unchanged.

If *expreſſed oils* are alſo ſecreted in the ſame manner, which I think not altogether impoſſible, conſidering how difficult of digeſtion fat is in many ſtomachs, and that thoſe who ſwallow much of it with impunity to the ſtomach, increaſe exceedingly in their own fat; if, I ſay, expreſſed oil, can be ſecreted on ſurfaces, we ſhall be able to account, in ſome meaſure, for the good effects of *oily mixtures* in inflammation of the lungs. I am more diſpoſed, however, to believe, that theſe effects depend on another principle.

The *juice of the cicuta*, when its freſh leaves have been applied to the abdomen, as a poultice, has alſo been taſted in the mouth.

Tobacco leaves, beat up with vinegar or brandy, and applied to the pit of the ſtomach, have in a few hours after produced vomiting. *Groundſel*, beat down into a very coarſe pulp, and applied cold to the pit of the ſtomach, has in the ſame manner produced the ſame effect.

With reſpect to the effects of *tobacco* and *groundſel,* it may be ſaid, that admitting the caſes to be ever ſo well founded, they do not *prove* abſorption. For a part, whoſe nerves are ſtimulated by ſome application, it is well known, may affect a diſtant part, and produce ſuch an action in that part, as if it itſelf had been alſo immediately ſtimulated by the ſame application. This conſent of parts is by the phyſiologiſts termed *Sympathy*. It has been much derided by ſome philoſophers, but is nevertheleſs a property of living matter, and as good *a term,* and *as intelligible,* in my opinion, as *attraction.* It may be compared with the vibrating of a cord not ſtruck, when it happens to be in *uniſone* with one which is ſtruck. No man can ſay why this takes place; we know it does take place, and are therefore perfectly contented. Aſk a muſician, Why one cord vibrates when the other is ſtruck? he ſays, It is becauſe they are in *uniſone.* Aſk a phyſiologiſt, Why one part is excited to action, in conſequence of a ſtimulus applied to another diſtant part, he ſays, It is becauſe they *ſympathize* particularly with one another. Now, certain applications to the ſkin may, by affecting its nerves, affect thoſe of the ſtomach, through ſympathy, though nothing be abſorbed from them. That the tobacco and groundſel required ſome hours to produce their effect, makes it more probable that their juices were *really* abſorbed. When parts are affected from ſympathy with other parts, the affection, almoſt always, takes place *ſuddenly*. We become *inſtantly* ſick from a bad ſmell, or a loathſome ſight; the raſping of a file *immediately* ſets the teeth on edge. The bladder *inſtantly*, in many habits, ſympathiſes with the ears in liſtening to the ſound produced in emptying a bottle of water, or with the ſkin in putting the hands into cold water, and gives an almoſt irreſiſtable inclination to let the urine go.

But to return to abſorption.—The body, we have been aſſured, has *weighed heavier* after it had remained ſome time in the warm bath; that it has alſo *weighed heavier*, during certain humid ſtates of the atmoſphere in the morning, than it was the preceding evening on going to reſt, notwithſtanding the quantity, which, according to *Sanctorius*'s experiments, muſt have been loſt by perſpiration during the night. Suppoſing the facts as here ſtated; we ſhall be obliged to allow that the increaſe of weight aroſe from an abſorption of the water of the bath, in the one caſe, and of the moiſture of the atmoſphere, or of air itſelf, in the other.

De Haen, on finding that patients tapped for the dropſy ſometimes filled again in a few days, was led to ſuſpect, that ſuch patients *abſorb from the atmoſphere.*

The paſſage of ſome poiſons and of infectious matter into the habit, ſtrongly demonſtrate *abſorption* from the ſurface of the body; this we ſee in the *venereal diſeaſe*; the *poiſon of the mad dog*; and the *matter* of the inoculated *ſmall pox*.

The courſe of the larger abſorbent veſſels, and the ſituation of the glands through which they paſs, and which are alſo parts of the abſorbent ſyſtem as we ſhall ſee bye and bye, are now very well known. When the abſorbent takes up a poiſon or matter capable of giving a particular diſeaſe, it commonly inflames; and becoming red, may be traced under the ſkin, running towards the neareſt glands. Having ſeen the tainted ſurface, or the wound into which the poiſon was inſerted, it is frequently a very eaſy matter to be able to foretell where theſe *red lines* will appear. When the glands have received the poiſon, they alſo inflame, ſwell, and not uncommonly, ſuppurate. During this period nothing very particular appears in the ſyſtem in general; but ſoon after, the marks of the poiſon, or of the particular infection, become very evident, and ſhew that it has contaminated the whole body. In this way we diſcover, not only that a poiſon, or infectious matter, is entering the body, but the very *road* it has taken.

The trunk of the body and its extremities, as well as the different viſcera, have two ſets of abſorbents, one, which run, on their external ſurfaces, and another, which is deeper ſeated. If a poiſon is abſorbed by the firſt, the *red lines* will generally be evident; if it happens to take the rout of the deeper ſeated ones, they will not be evident; and even if it is abſorbed by the ſuperficial ſet, unleſs it ſtimulates them very much, it will not conſtantly inflame them; of courſe they will not be *red*, will not be evident.

Let the infected ſore however be any where about the *head*, and if there are not *red lines* leading from it towards the ſides of the neck, *the glands of the neck*, at leaſt, neareſt the ſore, will almoſt certainly be inflamed, and will ſwell.

Let the ſore or wound be any where in the *arms*, or about the *breaſts*, the *red lines* will be ſeen running towards the *axilla*, if they are very ſuperficial; at any rate the *glands there* will be inflamed and ſwell. If the poiſon or infection is depoſited in any part of the *lower extremities*, or *about the parts of generation*, the *red lines* will appear running towards the *groins*, and the *glands there* will be inflamed and ſwell. Two circumſtances ſtill further confirm this doctrine. *The one* is, that if the gland is cut out while the poiſon is recently lodged in it, the diſeaſe will *frequently* be prevented. *The other* is, the ſucceſs attending the common method of curing the venereal diſeaſe, by rubbing mercurial ointment on and around the infected ſurface, or ſo as to ſend the antidote through the ſame veſſels which took up the poiſon.

I ſaid that the abſorbents alſo took up *ſolids*.

From *Du Hamel's* experiments, in feeding animals with *madder*, it appears,

That ſuch parts of the cartilages as were then oſſifying, became *red*;

That, though all the *growing bones* became *red*, thoſe which were intended to be the *hardeſt*, became *reddeſt*.

This laſt circumſtance makes it more than probable, that as bones derive their hardneſs from earthy particles uniting with the animal part, the *colouring part of* madder is a fine *earthy powder*; that this powder is at firſt abſorbed from the inteſtines by the lacteals, and afterwards depoſited in the bones by the arteries. From the ſame experiments it appears, that on leaving off the uſe of the *madder*, the bones of thoſe birds which had been fed with it, and which hitherto appeared as red as ſcarlet, even through the living ſkin, in a few months became perfectly *white* again. From this circumſtance we muſt infer that the *earth of bones* is abſorbed, and that the bones themſelves are perpetually changing.

If that part of the *urinary calculus*, which happened at the time to be forming in the bladders of ſome animals, ſubjected to ſimilar experiments, became red, in proportion as the bones became whiter, we have ſtill an additional proof, that the colouring part of madder is earth, and that *ſolids* are abſorbed.

That the earth is abſorbed from the bones, (and the animal part probably alſo), cannot be doubted, when we know that the thigh bone, for example, of a man at ſeventy, though little different in ſize, is three times lighter, is weaker and more ſpongy than it was at thirty, or than that of a young man: it certainly was once as heavy as the young man's; what is now become of the matter it formerly contained?

If the *calculus* became red as the bones became white, it becomes more than probable, that the earth, which is abſorbed from the bones, is that which is afterwards employed to form the *calculus*.

Phyſiologiſts have hitherto ſuppoſed, that when any of the ſofter or harder ſolids of the body were removed by *a diſeaſe*, that it was in conſequence of a *diſſolving menſtruum*, furniſhed for that purpoſe by the conſtitution. Thus *pus* and *blood itſelf*, have been accuſed of diſſolving, not only *ſoft parts*, but the *bones* themſelves. The idea was not altogether *unphyſiological*; ſince a very eminent modern *phyſiologiſt* allows, that our food is diſſolved by a *menſtruum* furniſhed by the ſtomach; and that after death, this *menſtruum* can diſſolve the ſtomach itſelf. From a variety of circumſtances, and comparing all the appearances together, Mr. *Hunter* is perſuaded, that the proceſſes of *digeſtion* and *ulceration*, (or *the removing* of *ſolids*), in no one circumſtance are like one another; that in the laſt there is no menſtruum; and that the *abſorbent veſſels* of animal bodies are, the only *counteractors* of the arteries; and alone concerned in removing parts; nor does he find it more difficult to conceive, that an abſorbent, in removing a bone, ſhould take up a particle of earth, than that an artery ſhould depoſite it, in forming a bone.

The *alveolar proceſſes*, as ſoon as the teeth have dropt out, are abſorbed in old men, the cavity of the mouth becomes leſs; hence the approximation of the chin to the noſe, the ſuperfluity of lips, and uncomfortable redundancy of tongue, obſervable in moſt old men. There was *no pus, no blood,* nor any *menſtruum,* which can be proved, which could diſſolve ſo large a portion of bone as an inch depth of each jaw, through its whole circumference.

The *waſting* of the fangs of a *long neglected dead tooth,* ſome months after it had been inſerted into a *living jaw,* in what is called the *tranſplanting of teeth,* ſeems to invalidate ſomewhat the former theory. So does the *waſting of the central part of* what is called *a dead bone,* after every living proceſs had ceaſed in its circumference; after it had become looſe, and was totally unconnected with the ſurrounding living bone. This ſometimes has happened in the *exfoliation,* for example, of nearly the whole of the *parietal bone.* As to the appearance of the tooth, I own I am unequal to the taſk of ſolving it. Mr. *Hunter* believes that the living veſſels not only abſorb *living animal ſolid,* but in certain circumſtances crowd on and abſorb the *dead ſolid.*

I have ſuſpected, however, that in the other inſtance, though the greater part of the bone might be deprived of life, ſome parts of it might ſtill continue to live. For bones do not receive their veſſels from one *centre of ramification*; there is commonly one or two large *ones*, but there are innumerable leſſer *ones*. The veſſels of the *perioſteum* communicate perpetually with thoſe of the ſoft parts above it; in which caſe, ſuppoſing perioſteum to be pretty generally detached by the accident which in part killed the bone, ſome portions of it may adhere, and be able, not merely to ſupport the life of certain parts of the bone under them; but may be equal to the active proceſs of *ulceration* and removal of parts in the bone. If *pus* was capable of diſſolving dead bone in general, it will be very difficult to give a reaſon why it ſhould fix on certain ſpots, and leave others untouched, when it lay equally over the whole ſurface.

II. Having endeavoured *to collect the proofs of human abſorption*, I ſhall next ſuppoſe that I am required *to give ſome account of the veſſels which perform this abſorption.* I have already mentioned *the abſorbents and their glands*, but in a general way. *Phyſiologiſts* themſelves might not be certain what particular veſſels I meant, for many have believed, and ſome ſtill believe, that the *common veins* only abſorb; or that if *lacteals* and *lymphatics* alſo abſorb, the office is divided equally between *them* and the *veins*.

Before the diſcovery of the *lacteals* by *Aſelius*, *certain extremities of the red veins* were ſuppoſed to be *the only abſorbents*. The *veins*, as was believed, had *two origins*; *one*, a continuation of the ſame tubes, of which the arteries were formed, but *reflected*; by *this*, the arterial blood was returned to the heart. The *other*, an aſſemblage of ſmall branches, which ariſing, by open mouths, from ſurfaces and cells, and ſoon joining thoſe which were continued from arteries, ſerved to abſorb the *chyle* and other fluids. After *this anatomiſt* had diſcovered that the *veins* did not abſorb the *chyle*, ſtill they were imagined to abſorb on every other ſurface except that of the inteſtinal tube; and that abſorption was performed there by an *uncommon ſet of veſſels*, was ſuppoſed ſomething peculiar to that ſurface.

The *lymphatics*, (or the *lacteals* of other parts), were next diſcovered by *Rudbeck*, twenty or thirty years after the *inteſtinal lymphatics* (for they are the ſame kind of veſſel) had been diſcovered; he appears to have entertained an idea, that *they* alſo, ſome how, *abſorbed* fluids. Speaking of his new veſſels, he ſays, " *intus, excavata & fiſ-*

tuloſa ſunt, infinitas habentia valvulas, ſemper veſiculam chyloſam, ſive ejus ductus, adſpicientes, ne humor a glandulis, vel aliis partibus, exſuctus, *iterum refluat*." And ſpeaking of this *humor*, afterwards, it appears he meant, *interſtitial fluid*, or the fluid of ſurfaces, for he calls it *humor calore exſudatus*. *Gliſſon* alſo appears to have had ſimular ideas; and as *Haller* ſays, "*a venis, hanc reſorptionem, ad vaſa lymphatica, tranſferre tentavit*."

I cannot admit that *Willis*'s conjecture, concerning the uſe of the lymphatic veſſels, conveys the *idea of abſorption*. He expreſsly ſays, the fluids come into the lymphatics in form of *vapour*, and are there condenſed into *lymph*, "*vapores a ſanguine accenſo, emanentes*,"——"*in aquam per hæc vaſa appropriata—condenſantur*." And ſtill keeping up the idea of *diſtillation* rather than *abſorption*, he bye and bye ſays of the ſame vapours, "*condenſatas, per lymphæductus*,

phæductus, quaſi per totidem alembici roſtra, extillent." *Bartholine* appears to have believed, that theſe *lymphatic veſſels*, like the red veins, were continued from the extremities of the arteries, and that when the arteries had carried the rich blood to different parts, for nouriſhment or ſecretion, theſe veſſels carried back the lymph which had been employed as *a vehicle* for the nutrient part of the blood. The greater number of *phyſiologiſts* ſeem to have acquieſced in this opinion; and though they allowed that the *lacteals* abſorbed a fluid, denied a ſimilar office to the *lymphatics*. *Dr. Hunter*, from comparing the *lacteals* and *lymphatics* together, obſerving that their ſtructure and appearance were the ſame; that neither of them were *eaſily* injected from the blood veſſels; and particularly from obſerving, that *poiſons* or *infectious matter* in entering the blood, always took the courſe of the *lymphatic veſſels* and their *glands*, taught, that *lymphatics* and *lacteals*, were the ſame kind of veſſels;

that *their common office was abſorption*; and that *this office was not divided between them and the red veins, but belonged to the lacteals and lymphatics only.*

Baron Haller, one of the greateſt phyſiologiſts we have had, though he admitted abſorption by *lacteals* and *lymphatics*, contended, however, that the *red veins*, *alſo* abſorbed. Other eminent anatomiſts were alſo of his opinion:— "*reſpondebimus interim* (ſays he) *multo amplius, reſorptionem patere, quam venarum lymphaticarum imperium; & ibi peragi, ubi nunquam, certa fide, ejuſmodi vaſa oſtenſa ſunt, ut in cerebro, pleura, peritoneo, & cute.*" He not only believed that *lymphatics* did not exiſt in many places of the body, but he thought he proved, from his injections of the *veins* in the dead body, that *they certainly opened at their extremities on ſurfaces*; and as they thus allowed the injection to eſcape on theſe ſurfaces, ſo they might alſo, by the ſame orifices, abſorb from theſe ſurfaces: "*neque raro vidi* (ſays he) *cæruleam ichthyocollam, quæ de venis, exhalaverat, pericardii figuram expreſſiſſe.*"—"*Et iterum, figuram venticulorum cerebri, glutine piſcium, per venas impulſo, non ſemel conſervatam vidi; ut manifeſtum ſit, a venis, in eas omnes caveas, liberum iter eſſe.* *Haller*'s authority is, beyond *controverſy*, reſpectable; but his injections were thrown into *dead bodies*, which, we know, allow fluids to *tranſude*. Mr. *Hunter*, maintaining the ſame opinions as his *brother*, with a view to decide this curious point, made a variety of experiments on living animals; and proved, in my opinion, that the *red veins* do not abſorb on the *inteſtines*, and made it therefore more than probable, that they abſorb *no where elſe*. On no occaſion did he find, that the *veins* took up *chyle*, *coloured fluids*, or *ſolution of muſk*, purpoſely thrown into the cavity of the inteſtines; nor, on the other hand, when he injected the *meſenteric veins,* did he find, *that any thing eſcaped from their extremities into the cavity of the inteſtines.*

I cannot help ſaying, that when *Haller* informs us, that *there are no lymphatics in the brain, becauſe they have not been ſeen,* and that *red veins* muſt therefore abſorb in the *brain,* he infers too much. *Haller* and *Senac* have informed us, that they never could diſcover *lymphatics* on the *human heart*; where I have been fortunate enough to inject *ſome thouſands.* The *brain* becomes ſoon, after death, ſo ſoft, that it will not bear injection. In the young animal, it ſeems little elſe than a *thick fluid.* One intention of this, perhaps, is, that it may better bear *compreſſion in the birth,* than it would have done had it been more *ſolid.* Maceration in water is the method by which the abſorbents of parts may generally be detected; but if the *brain* is, of itſelf, ſo tender as not to be handled, or ſpoils ſo quickly as not to bear maceration for a ſufficient length of time, and if *lymphatics* are with difficulty diſcovered in very firm parts, no wonder we have been ſo unſucceſsful in attempting to find the *lymphatics* of the *brain*. I have ſeen *abſorbent glands* in the *foramen caroticum* in the baſis of the ſcull: they could not poſſibly be there, unleſs there were abſorbent veſſels, to which they belonged, alſo paſſing that way, and coming from *within the ſcull*. If we diſcover abſorbents in the right leg, we may ſurely infer, from the known uniformity of Nature in her works, that they alſo exiſt in the left, though we have not ſeen them. The office of the *veins* is, *to return the blood from the arteries to the heart*; the office of the *lacteals* and *lymphatics* is evidently *abſorption*. Why ſhould we ſuppoſe, that when one ſet of veſſels are expreſsly formed for abſorption, a double taſk ſhould be impoſed on the *veins*, whilſt, at the ſame time, the numbers of the confeſſedly abſorbing veſſels demonſtrate that they muſt perform ſome great and important office in the machine?

III. Having ſet out with the idea, that the *human abſorbents* took up *mercury* in the form of a fine powder, *ſome account of their orifices,* will alſo be expected. This unavoidably leads me into a very curious and extenſive field. Almoſt all the *anatomiſts,* hitherto, have been perſuaded, that *theſe orifices were too minute* to be viſible even in the microſcope; that *this minuteneſs of their orifices* anſwered a very good purpoſe, and ſerved as *a guard* on the conſtitution, to prevent any thing *coarſe* from entering the blood. Whoever ſets out in ſearch of theſe orifices, will moſt naturally be led to the *internal ſurface of the inteſtines.* He muſt perceive, that his probability of ſucceſs will be greater on that ſurface than any where elſe in the body. The different veſſels are there elongated into *proceſſes,* which, to the naked eye, ſeem, like hairs, or the pile of velvet, to project from this ſurface, and are therefore termed *Villi* by the anatomiſts. Each *villus* has its little *artery*, *vein*, and *abſorbent*; though we cannot demonſtrate, *anatomically*, yet the *colic* convinces us ſufficiently, that they alſo have *nerves*. Theſe veſſels and nerves are connected by cellular membrane, and have alſo a kind of *cuticular covering*.

Haller ſays, this *cuticular covering* is eaſily demonſtrated *in the great inteſtines*. I have ſeen it on the ſmall inteſtines. I own I doubted of the truth of their doctrine who aſſerted, that *cuticle* was continued from the *mouth* to the *anus*. I have ſeen the cuticular lining of the *œſophagus* evidently terminate within the *cardia*, by a regular *border*. I have ſeen *this* in the human ſubject. It has been long ago obſerved in the *aſs*. There is an appearance which might lead one to imagine, that it even terminated in ſome animals in that part of the *œſophagus* which is immediately below or oppoſite to the *larynx* as in the *cat*. This laſt inſtance is certainly a *deception*. It is *a particular fold*, but no ways reſembling the *cuticular* termination in the *cardia* of the *aſs*. I will not ſay, that the *cuticle* does not terminate a little within the *ſtomach* in ſome animals. But I am convinced, that the *rete mucoſum*, one *lamina* of which I can demonſtrate to be in every reſpect like the *cuticle*, is continued over the inteſtinal tube, and covers the *villi*. *Rete mucoſum* ſeems to be wanting in the *ſole of the foot*, and to terminate ſoon within the *lips* and *noſe* in the *Negroe*. I know it does not *terminate* there, but only *changes its colour*. The conſtituent parts of the *villus* are not ſurely connected and covered by *cellular membrane only*. If this were the caſe, the *fluids* and *flatus* of the inteſtines would, from their *periſtaltic motions*, be forced into this *cellular membrane*, and pervade the whole body.

In the *dog*, the comparing the *villi* to *hairs*, is not altogether improper; but viewed in the microſcope in *men*, they reſemble more a *tranſverſe ſection* of the end of one's finger, were the finger not quite ſo round, but flatter before and behind, of courſe not ſo thick on the ſides. This ſection of the finger muſt alſo be ſuppoſed diminiſhed, almoſt to the ſize of a pin's head, the *villi* will then have two broad ſides, and two narrow ones; one looſe floating oval edge, and one ſtreight fixed under edge. Each villus is a kind of *valvula connivens*, in miniature, and, like it, intended to increaſe the ſurface.

I had learned, that *Leiberkuhn* had ſeen the *orifices of the lacteals* on the tops of theſe *villi*. Mr. *Hewſon* at one time informed us, that he alſo had diſcovered the orifices of the lacteals on the inteſtines of the *gooſe*, and that *they began by a pair of valves*. He appears to have deſerted this opinion, in his laſt publication; and having ſeen *ſomething* of the appearance *Leiberkuhn* deſcribes, talks of the abſorbent orifices *in a very vague manner*. In one page he ſays, "I *think* I can *clearly* ſhew the orifices of the *lacteals*;" in the next, "the extremity of the *villus ſeemed* ſpongy and porous;" and bye and bye he ſays, "Having, on repeatedly examining them, obſerved *the pores, or orifices, very diſtinct and empty*."

I was ſatisfied with neither authority. —Tho' I had frequently looked for them, I never ſaw any thing like *the orifices of lacteals*. At laſt, about a year and a half ago, a very favourable opportunity of inveſtigating *theſe orifices* preſented itſelf. A woman had died *ſuddenly*, about four in the morning, after having been in pretty good health on the preceding evening. Her relations wiſhed to know the cauſe of ſo ſudden death; and the body was opened. The *lacteals*, on the outſide of the inteſtines, and along the *meſentery*, were more turgid, with a *firm coagulated chyle*, than I had ever ſeen them. *They* were alſo, in proportion, more numerous, and gave an idea of their being, at leaſt, equal in number to either *arteries* or *veins*. The *menſenteric glands*, inſtead of putting on their uſual *reddiſh* appearance, were, from the quantity of *chyle* they contained, *perfectly white*. When the inteſtine was opened, there appeared a number of *white points* up and down its internal ſurface; in ſome places they were crowded together, but in general ſcattered. Theſe *points*, on cloſer inſpection, would have made one ſuppoſe that each *villus* was a *veſicle* turgid with *chyle*. The arteries and veins were ſo compreſſed from the diſtenſion of the extremity of the *lacteal* by the *chyle*, that the whole *villus* ſeemed to belong to the *lacteal*. Though other *villi* were not equally white, or diſtended, I afterwards found they all contained *chyle.* In a portion of this inteſtine, ſubjected to the *microſcope* in a ſtrong light, I ſaw diſtinctly a number of *pores* on the tops and ſides of thoſe villi; I was convinced they were the *abſorbent orifices of the lacteals.* They appeared to be in a kind of *bulbous extremities* of thoſe veſſels, and their diameters, as I had often ſuppoſed, *a priori*, were, in reality, ſeveral times larger than thoſe of the *particles of the blood*, ſeen under the *ſame microſcope.* I had a drawing made of ſome of the more diſtinct turgid *villi*, and of *theſe abſorbent orifices. Dr. Hunter, Dr. Jebb*, and ſeveral of my anatomical friends, were occaſionally preſent, ſaw there appearances, appeared to be ſatisfied; nor can I ſuppoſe that I was deceived. The reaſon, probably, of my ſucceſs here, was, that the *chyle* coagulates in the dead body; and gives that *erection or diſtenſion* to the *villi* which they had, when under the *ſtimulus* of abſorbing. Without this they *collapſe*, particularly in the dead body, and the *orifices* become inviſible.

I next ſet about diſcovering *theſe orifices* in the *ſkin*, but with little hopes of ſucceſs. The extremities of the veſſels do not *there*, as in the inteſtines, form *villi*; except on particular parts, as on the *lips*, *tips of the fingers and toes*, *palms of the hand*, *or ſoles of the feet*. Or if the veſſels do form *villi* in other parts of the *ſkin*, theſe are ſo ſhort, ſmall, and crowded, as to make the ſurface ſeem ſmooth. Where the *villi* are long they are ſtill ſmall, compared with thoſe of the inteſtines. The *lymph* is never, perhaps, abſorbed with ſuch velocity, or in ſuch quantity, in a given time, as the *chyle*; or ſo as to make the *villus* of the ſkin as turgid as the *inteſtinal one*. But ſuppoſing it is, we cannot determine the time of this *turgeſcence* as in the inteſtines; and if we could, the *lymph* is ſtill tranſparent, and therefore can never give that diſtinctneſs, which a white coagulated fluid produces, in the *villi* of the inteſtines. *Leiberkuhn*, we are informed, in order that he might be able to find the *orifices of the lacteals*, gave his patients *milk* to drink before death, and found it afterwards coagulated in the *villi*; but had he bathed the *hands* and *feet* in milk before death, I doubt if the *abſorbents of the extremities* would *then* have taken it up, or if they had, and had taken it alſo in conſiderable quantity, ſtill I believe it would not have coagulated, nor made the *villus* turgid, as it had not paſſed through theſe previous changes in the ſtomach and inteſtines, which convert it into *chyle* and diſpoſe it to coagulate. The inteſtines appear *to live* after the other parts of the body, *are dead*, and abſorption frequently takes place from their internal ſurfaces, after the animal has *expired*; but there are no facts, which tend to prove abſorption in the *extremities* in the recently dead body; they are parts which generally *die* firſt. The ceaſing of the *pulſe* in the *wriſt* before it ceaſes in the *middle of the arm*, proves this, and ſhews that the *blood* is retiring to the *internal parts*; whilſt the *ſubſultus tendinum* equally ſhews, that the *brain* is alſo beginning to withdraw its *ſtimulus*.

Though I did not ſucceed in finding *the abſorbent orifices* on the *villi* of the *ſkin*, I had formerly *injected the abſorbents of the ſkin with quickſilver*: I had not the leaſt doubt of their being there. I was alſo led to make ſeveral obſervations on the *pores of the ſkin*, on the *cuticle* and *rete mucoſum*, which, as connected with the manner in which I ſuppoſe *abſorption* begins on that ſurface, may not be *impertinent* here.

Remarks on the Skin, and its pores.

WHEN a *bliſter* has been applied to the *ſkin of a negroe*, if it has not been very ſtimulating in twelve hours after, a *thin* *tranſparent greyiſh membrane* is raiſed, under which we find a fluid. This membrane is the *cuticle* or *ſcarf ſkin* When this, with the fluid, is removed, the ſurface which was under them appears *black*; but if the *bliſter* had been very ſtimulating, *another membrane*, in which this black colour reſides, would alſo have been raiſed with the cuticle, this is *rete mucoſum*, which is itſelf double, conſiſting of *another grey tranſparent membrane*, and of a *black web*, very much reſembling the *nigrum pigmentum of the eye*. When *this membrane* is removed, the ſurface of the true ſkin, *(as has hitherto been believed,)* comes in view, and is *white*, like that of a *European*. The *rete mucoſum* gives the *colour* to the *ſkin*; is *black* in the *Negroe*; *white*, *brown*, or *yellowiſh*, in the *European*. The reaſon why this membrane is *black* in the *Negroe*, is, perhaps, that his body may be better able to defend itſelf againſt *the ſun's rays*, and that the *heat* may be prevented from penetrating The intention of a ſimilar membrane behind the *retina* in the eye, appears to be, not only that of *abſorbing* the ſuperfluous rays of light; but according to the *ingenious optician, Mr. Storrer,* like the *amalgam* behind the *looking-glaſs,* of enabling the *retina* to *reflect the rays* in order to perfect viſion. It is not very improbable that ſome ſuch purpoſe as enabling the *cuticle* to reflect the ſun's rays in thoſe warm climates, where the inhabitants originally go naked, may be the intention of nature, in giving them the *black membrane.* Perhaps too, the circumſtance of the countenance's becoming *brown,* when expoſed to the ſun's rays in ſummer, in our own climate, may be a proceſs of nature to defend herſelf againſt the acceſs of external heat into the body.

Both *cuticle* and *rete mucoſum* ſend innumerable *proceſſes* into the *pores* of the true ſkin; the *proceſs* of the *rete mucoſum* is always within that of the *cuticle,* and in contact with the ſides of the *pore,* as formed by the true ſkin. Theſe *proceſſes* are remarkable in the *cuticle* and *rete mucoſum* of the *elephant,* and ſome of them are almoſt an inch long; the *cuticle,* or *rete mucoſum,* or a membrane very ſimilar, and having the ſame properties with theſe, appears to me to be alſo continued into the inſide of the *mouth* over the *tongue, internal ſurface of the lungs, œſophagus, ſtomach,* and *inteſtinal tube.* In moſt of the laſt named parts, the *cuticle,* however, forms *ſheaths* for *villi,* and not *proceſſes* which line pores. On viewing the ſurface of the ſkin, even with the naked eye, we find it *porous;* more ſo in ſome places than in others; and that the *pores* are alſo larger in ſome parts than others. Theſe *pores* are either *ducts of ſebaceous glands; ſerve to tranſmit hairs;* (and in my opinion,) *the greateſt part of the perſpirable matter itſelf. Abſorption on the ſkin* alſo, in all probability, *begins on the ſides of theſe pores.* They are particularly remarkable about the *mouth, noſe, palms of* *the hand, ſoles of the feet*, on the *external ear, ſcalp, mons veneris*, and *around the nipple* in women. *Grew* thinks he was the firſt who obſerved them on the *fingers*; and has given a pretty juſt *engraving* of them, in the *Philoſophical Tranſactions*. *Winſlow* deſcribes theſe laſt, and ſays, they are the *ducts of glands*. The proceſſes which line the pores tranſmitting hairs have been long obſerved, but I do not recollect that any *anatomiſt* has deſcribed theſe proceſſes which line the other claſſes of pores. *Albinus* takes notice of the appearance, but ſays, (if I am not miſtaken), that they are the *roots of hairs* pulled away with the *cuticle* or *rete mucoſum*. *The proceſſes which line the pores*, would however, from what I can collect of the opinions of the *moſt eminent latter anatomiſts*, be reckoned *imperforated*, and deſcribed as ſo many *blind pouches*, reſembling the fingers of a glove, which might be pulled out of the *pores* entire, by long maceration of the ſkin in water. Of courſe the *cuticle* and *rete mucoſum* would, in their opinion, be reckoned *every where entire*; and it muſt be owned, that when theſe *membranes* are ſeparated by *maceration*, and viewed in the *microſcope*, there is not the leaſt appearance of *pores*. *Haller*, *Albinus*, and *Meckel*, are of opinion, there are no pores in theſe membranes. None of the latter *anatomiſts* have been able to diſcover *Lewenhoeck*'s *ſcales*, of which he believed the *cuticle* was compoſed, and between whoſe *interſtices*, or *looſe edges*, the fluids paſſed into the body, or paſſed out. *Malpighi*'s and *Ruyſche*'s *perforations* of the *rete mucoſum* have been ſought for with no better ſucceſs. I own, that after ſome pains, and aſſiſted by pretty good *microſcopes*, I have not been able to diſcover *perforations* in the *cuticle* or *rete mucoſum*. It is true, that by macerating the *tongue of a calf* in water for a conſiderable time, *an appearance of pores* may be produced in the *rete mucoſum*; and it is as true that *the ſame appearance* may be produced in the *cuticle.* But when the *one* appears *perforated,* the correſponding ſurface in the *other* is always not ſo; and where the *proceſſes* are ſhort, and eaſily ſeparate from one another, neither *cuticle* nor *rete mucoſum* appear to be *perforated*; and both may be demonſtrated to have their own *proceſſes.* *Malpighi* firſt taught that the *rete mucoſum* was *porous.* I find *Haller* of the ſame opinion; alſo aſſerting from *Du Hamel,* that the *rete mucoſum* in the *feet* of many *birds* (he particularly inſtances the *Oſtridge)* is *perforated.* I have ſeen thoſe talked of *perforations,* and am convinced, that as in the *tongue of the calf,* theſe are only *vaginulæ,* or ſheaths for the *villi,* and cannot be demonſtrated by any means to be open at top.

Though I have not found *pores* in either *cuticle* or *rete mucoſum,* I believe nevertheleſs, that *they* certainly exiſt, and for reaſons which I ſhall give preſently;

but as I never could ſee *them*, I think it juſtice to thoſe gentlemen who admit nothing which is not *demonſtrable*, to ſay ſo. *Albinus* and *Meckel*, particularly the *laſt*, are diſpoſed to believe, that whatever fluids are *perſpired*, or whatever are *abſorbed* by the ſkin, muſt equally *ſoak* through the *cuticle*, as the *vapour of warm water does through dried leather*. *Albinus* even doubts, whether the *perſpired fluids* do not *ooze* through the *coats* of the extreme arteries themſelves, as vapour, and are afterwards condenſed into *ſweat*. "*Quid ni* (ſays he) *penetraret, per mollia noſtra, humidaque, quum calentis aquæ vapor, per durum, ſiccumque corium, eo modo penetret?*" *Profeſſor Meckel* uſes nearly the ſame language. Talking of the *cuticle* in the *memoirs of the Academy of Berlin*, he ſays, "*Quoiqu' inacceſſible aux vaiſſeaux, ſa nature eſt pourtant telle, qu'il tranſmet le liquide, dont il eſt imbû, a peu près, comme pourroit le faire un cuir mince humectè.*" *He* alſo obſerves, that though in the palms of the hands of *black-ſmiths*, and in the ſoles of the feet in *travellers*, the *cuticle* conſiſts of many layers, and is ſometimes a quarter of an inch thick, ſtill perſpiration takes place on theſe ſurfaces. Did the *fine perſpiring veſſels* reach the *cuticle* of the *foot* in the one inſtance, or of the *hand* in the other, the *weight* of the body, or the recoil of the hammer, he thinks muſt cruſh them to pieces.

Notwithſtanding of ſuch *reſpectable oppoſition*, I cannot help being perſuaded, that ſuch a proceſs as *ſoaking*, however it may take place in dead animal ſubſtance, or vegetable, is a proceſs too much allied to thoſe of *dead matter* to have any place in a *living body*. Nay, I think it may be proved, it never does take place in *cuticle*, even in the dead body. There are difficulties, however, on both ſides. Let us examine the different facts.

The reaſons which induce me to believe that there are *pores organized*, connected with the extremities of the *exhalent arteries*, in the *cuticle* and *rete mucoſum*, which, however inviſible in the *dead ſeparated cuticle*, ſtill exiſt, and are ſufficiently dilated in the *erected ſtate* of the extremities of the veſſels of the *living* and *perſpiring ſkin*, are the following:

When a piece of *cuticle* falls off from the *cutis*, ſome of the *hairs* go with it, and ſome remain with the *cutis*. Thoſe *hairs* certainly *perforated* the *cuticle*, yet, in the *microſcope*, not the leaſt veſtige of theſe *perforations* can be traced. In places where the *hairs* either do not exiſt, or where they are inviſible, *where*, however, the *pores* are very numerous, as on the *noſe* and ſome parts of the *external ear*; no *perforations* can be traced in the *ſeparated cuticle*; though the *ſebaceous matter* could formerly be preſſed from the cavities of theſe *pores* on the *noſe*, in form of a ſmall worm, of ſome conſiderable length. The *proceſſes* themſelves are frequently *tore off*, and remain with the pores of the *cutis*, yet no appearance of *perforation* is ſeen in the ſeparated *cuticle* of any ſuch part of the ſkin. I perforated pieces of *cuticle* with a *fine needle*, but theſe *perforations* were *inviſible* in the *microſcope*, as they would have been had I perforated the *elaſtic gum*. The pores of *filtrating paper*, when dry, are very manifeſt in the *microſcope*; but on wetting this paper, they become inviſible. The *dead cuticle*, and even the *callous living cuticle*, ſwell from water, though the ſound parts of *living cuticle* do not ſeem to undergo any change from lying *long* in water. The *cuticle* of the *palms of the hand*, and of the *ſoles of the feet*, ſeem at leaſt to imbibe moiſture; but the *cuticle* on the *oppoſite ſides* of the hands and feet do not appear to have undergone any change. If *dead cuticle* ſwells in water, its *pores* will inevitably become inviſible. I ſhall, bye and bye, offer ſome reaſons, for making it probable at leaſt, that the *firſt perſpiring* and *abſorbing pores* are in the *proceſſes* or *vaginulæ* of the *cuticle* and *rete mucoſum*, and that thoſe which appear on the outſide ſurface are *ſecondary*, reſemble *mucous ducts*, and are common to a vaſt number of the *primary pores*. Farther reſpecting the *ſoaking of fluids* through *cuticle* and *rete mucoſum*, let it be remembered, that in many *fevers* the ſkin is for a long time *parched* and *dry*, though it *looks red* and *feels hot*; the laſt circumſtances prove, that the *blood* is derived to the ſkin in greater quantity than at other times, yet the fluids do not *ſweat* out, and much leſs *tranſude*. Many people, notwithſtanding their uſing exerciſe, even in hot weather, when the fluids muſt be determined to the ſkin, do not ſweat. I have ſeen *veſications* take place from *burns*, from other accidents, or from the conſtitu-

tion;

tion; theſe have been left to themſelves; the fluid has not appeared ſenſibly to evaporate; they have remained, apparently, of the ſame ſize, for eight or ten days, without the *cuticles* ever feeling *moiſt*. When a bit of ſkin, with its *cuticle* ſound, and adhering, is expoſed to air, it will be many weeks in *drying*; and were not the cuticle to ſeparate by *putrefaction*, would probably never dry at all. I expoſed ſuch ſkin to the heat of 100° or 120° for two days, without its appearing to have dried in the leaſt. When *cuticle* happens to be rubbed off, the ſkin dries *immediately*. Though the legs in *œdema* are loaded frequently with *lymph*, not a drop *tranſudes* through the cuticle, unleſs the diſtenſion has been ſo great as to tear it, which rarely happens. Is it probable that the *ſame cuticle* ſhould be the moſt *permeable* and the moſt *impermeable*, to fluids of any ſubſtance, at one and the ſame time?

You yourſelf, it may be ſaid, allow of pores; why, if theſe pores exiſt, does not the fluid of *veſication* eſcape by the pores, though it may not tranſude? Theſe pores, I have already ſaid, I believed were in the *proceſſes of cuticle and rete mucoſum, which lined the pores of the ſkin*. If one preſſes his finger about the middle in hot weather, or applies a ligature, the *perſpirable matter* will be forced out at the pores on the tops of the fingers, in *round drops*, at regular diſtances, on the *ſpiral ridges*, like the ſecretion of the *tarſal glands* of the *eyelids*, after they have been immerſed in ſpirits. In the *latter caſe*, the equal preſſure of the ſurrounding fluid may oblige the ſecretion to put on the appearance of *round drops*. But I will not admit *Albinus*'s reaſoning as juſt, when he ſays, the fluid perſpires, in the *former inſtance*, from every part of the ſkin, and is collected into *drops* by the equable preſſure of the ſurrounding *atmoſphere*. I ſee the drops appear at the *orifices* of the pores, and no where elſe; and their rounded form, depends on their being accumulated in a round cavity, the *orifice of the ſecondary pore.* This makes it more than probable, that the *perſpiring pores*, and from analogy, the *abſorbing pores*, are in the *proceſſes* of the *cuticle* and *rete mucoſum*, which line the *ſecondary pores* of the *cutis*, and not in that *apparent* external interſtitial ſurface of the cuticle itſelf, placed between the mouths of the external pores. What further confirms this idea is, that the parts moſt porous *ſweat* moſt, and, I dare ſay, will be found to *abſorb* moſt. The *tip of the noſe*, in warm weather, the *head*, the *arm-pits*, the *ſoles of the feet*, and *palms of the hands*, ſweat moſt. Now, though I contend that there are pores in the *cuticle* and *rete mucoſum*, ſtill I think it poſſible to give a reaſon why the cuticle does not allow the fluid of *veſication* to eſcape.—When cuticle is detached by *veſication*, its proceſſes muſt be compreſſed againſt its internal ſurface, and the pores of courſe will be ſhut. When *œdema* diſtends a limb, the fluids do not eſcape for another reaſon. The extreme arteries, which exhale on the ſkin, are, probably, compreſſed by the water, and the cellular membrane become turgid, by exhalation, from more internal branches: and beſides, *ſweating* is a ſecretion which I cannot conceive conſiſtent with the diſtended ſtate of the *cold* ſkin; we have therefore no moiſture, in general, from ſuch ſurfaces. I have ſaid, that though I expoſed ſkin to heat, ſufficient to convert its fluids into vapour, (and as vapour is allowed to be more penetrating than fluid, it ſhould of courſe have dried quickly), that it did not. Now, if the *villi* are either ſuppoſed to be *collapſed* or the *proceſſes compreſſed*, one may ſee *ſome reaſon* why no moiſture appeared on the ſkin. A ſtate of erection, diſtenſion, and perfect freedom, may be neceſſary to *perſpiration*, and eaſily obtained in the living body; but from the relaxation or compreſſion of the *villi*, any proceſs ſimilar to perſpiration may be impoſſible, notwithſtanding the action of *heat*, which could not make theſe veſſels exert a power conſiſtent only with *life*. The ſurface of the *cuticle* is always covered with an *unctuous*, or *oily ſecretion*; this is very conſpicuous in the ſkin of the *Negroe*, and makes it ſtill more improbable, that watery fluids *ſoak* through it: this may be one reaſon why it does not ſuffer the *cutis* to dry,—though I doubt it. The cuticle of the hands and feet, I allow, in the living body, ſeem to imbibe moiſture, and become ſofter; but it is probably, in conſequence of its having *leſs living principle* than that of other parts.

That it allows of the *ſweat's* paſſing through, may be eaſily accounted for, though the *ſoaking of fluids* through it ſhould be denied; for admitting that in the palms of the hands, or ſoles of the feet, there may be *many layers of cuticle*, ſtill it is moſt probable that the laſt formed correſponds in every reſpect to the firſt formed and intermediate *layers*, and that *pores* are oppoſite to *pores*, and connected with each other.

Beſides, the *villi* appear to be *lengthened*, as the *cuticle* becomes thicker; I do not mean to ſay, that they were not originally intended to be long on the fingers and toes, and that thoſe parts are not proportionably more vaſcular, even in the *fœtus*, than almoſt any other part of the ſkin, but it is equally probable that the *villi* there were originally endowed with a property of *elongating* themſelves in proportion to the neceſſity, ſince thoſe parts, expoſed to *greater friction*, would of courſe conſtantly be covered with a *thicker cuticle*. The *villi*, I know, are ſuppoſed to be longer there, for the ſame reaſon as on the *lips*; that is, for the purpoſe of more *exquiſite ſenſation*. For though I have obſerved, that the greater part of the *villus* conſiſted of *blood veſſels and abſor-*

bents,

bents, yet theſe have been ſuppoſed to be elongated on the account of the *nerves*. I ſhould rather believe that ſeveral purpoſes, beſides the accompanying the *nerves*, might be anſwered, by the *elongating* of the *villus*, and that a greater *perſpiration*, for inſtance, as well as greater *abſorption*, takes place on thoſe ſurfaces. That they may be capable of *furniſhing* a *thicker* and more conſtant *ſucceſſion* of *cuticles*, is perhaps alſo part of the intention of *Nature* in forming them ſo *large* there. I have not ſeen any *villi* longer than thoſe in the feet of *hooved animals*, as in the *horſe* and *cow*, and the correſponding pores in the *hoof* are equally deep. Even in the *ſlink calf*, theſe *villi* penetrate ſo deep into the *hoof*, that when the *foot* has been ſucceſsfully *injected*, and the *hoof* afterwards ſeparated by *maceration*, many of the *tore villi*, adhering in the *pores* of the *hoof*, have given it the appearance of being *injected*. On the *tongues* of *quadrupeds*, where the *cu-*

ticle and *rete mucoſum* are much thicker than in any part of the human body, the *villi* are alſo larger and longer.

If the veſſels elongate as the cuticle thickens, it will be ſaid, what is the uſe of the cuticle's thickening at all in the *palms of the hands* of hard working people, or in the *ſoles of the feet* of thoſe who walk much. It has been preſumed, that it becomes thick, in theſe inſtances, in order to defend the tender veſſels underneath from the effects of *preſſure*, or *violent concuſſions*. I believe it does; (though I could conceive it *merely diſeaſe)* yet I would not deny that a porter's hand has as delicate a ſenſation of *touch* as a *lady's*. Though the veſſels elongate as the cuticle thickens, ſtill they will be better ſupported in paſſing through a thick elaſtic medium, and better able to reſiſt the effects of preſſure, eſpecially as the *cuticle*, in thickening, becomes more *elaſtic*. That the veſſels of the ſkin may be ſtill more

defended

defended againſt this preſſure, is the reaſon we find ſuch a quantity of *cellular ſubſtance* behind it; as for example, on the *heel, ball of the great toe,* and *buttocks.*

That *elaſticity* in parts enables them to reſiſt violence, need not *here* be explained. A man may catch a *cricket-ball,* if it flies with ever ſo much force, providing his hand *yields* on receiving it; if the *fixed* hand, on the contrary, were to receive the flying ball, it might ſhatter every bone in it.

Dr. *Hunter* has deſcribed and delineated, in the *London Medical Eſſays, white filaments* paſſing between the *cuticle* and *cutis.* Theſe are moſt remarkable, in the *ſole of the foot,* in the human ſubject. He ſuſpects them to be *veſſels of perſpiration,* continued even to the *cuticle.* If they are veſſels, it correſponds with my idea of veſſels becoming larger and longer, in proportion as the *cuticle* becomes thicker. For theſe *filaments* are more eaſily demonſtrated on the *heel*, or *ball of the great toe*, where the cuticle is *thickeſt*, than any where elſe. We have been informed, that it has lately been diſcovered, that theſe *filaments* were *nerves*. That the *nerves* never become larger, but on account of more *acute ſenſation*, or *greater action* in a part, appears to me a ſufficient reaſon for rejecting the idea of *larger nerves* going to an *inſenſible* and nearly *paſſive* membrane. If theſe *filaments* are not *veſſels*, from *analogy* to the other parts, of the internal ſurface of *cuticle*, I ſhould rather ſuſpect they were exceeding fine *proceſſes* of the *cuticle* and *rete mucoſum*, which line the *ſmalleſt pores* of the true ſkin; and if theſe proceſſes are *elongated*, and go *inwards*, as the cuticle thickens, while at the ſame time they ſerve the ſame purpoſes as the *ducts of glands*, it comes to the ſame thing as if more of the veſſels themſelves had been *elongated outwards*.

If theſe *filaments* are really *proceſſes* of the *cuticle* and *rete mucoſum*, then I can demonſtrate *three claſſes of proceſſes* in theſe *membranes*. The *firſt* line the *pores*, through which the *hairs* paſs; theſe are the *longeſt*, and generally have the *largeſt diameter*. The *ſecond claſs* are eaſily diſtinguiſhed on the inſide of the *cuticle* which covered the *palms of the hands or ſoles of the feet*, (or indeed on any part of cuticle;) they line thoſe *pores* deſcribed by *Grew*, and which *Winſlow* calls, *the ducts of glands*; they are ſhort, compared to the former, are tranſparent on the ſides, and have a *white line* in the centre, which I do not well underſtand; they appear, in regular order, on thoſe parts of the *cuticle* which correſpond to the *parallel*, or *ſpiral ridges* of the *cutis*. The above mentioned *filaments*, *perhaps*, conſtitute the *third claſs*, are *longer* than the laſt, and more *ſlender* than any of the former.

In order to make it probable that *cuticle* is a ſubſtance, which may be pervaded by fluids, though it has *no pores*, *anatomiſts* have adopted one of two *theories*, reſpecting its formation. The *firſt* is, that it conſiſts of the *callous extremities of the veſſels of the ſkin*, reduced to this ſtate by the *friction*, which perpetually takes place between the ſurface of the body and ſubſtances coming in contact with it. *Morgagni* adopted this opinion.

The *ſecond* is, that cuticle and *rete mucoſum* were originally and ſtill are *exſudations of mucus from the ends of the veſſels of the ſkin*; that this *mucus* was dried and hardened by the external atmoſphere into *a membrane*. This laſt opinion has been ſupported by *Profeſſor Meckel*, who obſerves, in confirmation of his opinion, that the *black membrane*, in the *rete mucoſum* of the *negroe*, may ſtill be diſſolved in water, like *mucus* by *maceration*. I cannot perſuade myſelf to be of either opinion. There is ſomething elſe in *cuticle*; nor does its known properties correſpond with theſe theories. If the friction of external ſubſtances rendered the ends of the veſſels of the ſkin *callous*, whence have we *cuticle* ſo perfect, in the earlieſt ſtate of the tender *fœtus*, hanging in a *warm liquid*, more fit for *diſſolving*, as one would imagine, than *producing calloſity*? If the *cuticle*, on the other hand, is merely *concreted mucus*, whence ſhould it remain months in water without *diſſolving*, or becoming *putrid*? The *hooves*, *nails*, and *cuticle*, of animals, are ſuppoſed to be *ſimilar ſubſtances*, and always come away together after *maceration* in water; yet the *hoof* in the *ſlink calf* is almoſt an *inch thick*, while the *cuticle* is nearly the ſame as it is afterwards in open air.

I formerly mentioned, that the *cuticle*, unlike dried *mucus*, neither in the living nor dead body, admitted of the *tranſuding* of fluids. Dr. *Hunter* obſerves, that the *fine membrane* in the *rind* of fruit, ſuch as *lemons* and *oranges*, has the ſame property, as is demonſtrable from the drying and ſhrinking of the fruit, when this membrane is removed, whereas, if it is kept entire, the fruit may be preſerved for many months.

I cannot well ſuppoſe any part of the ſkin of a living animal *inorganic* and *not poſſeſſed of life*. If the *cuticle*, tho' an inſenſible membrane, were not *alive*, and poſſeſſed of *irritability*, why ſhould touching it with *cauſtic*, which deprives other parts of *life*, and makes them drop off, have the ſame effect on the *cuticle*?

If a bit of *cuticle* is touched ſlightly with moiſt *lunar cauſtic*, it ſoon becomes *black*, and in a day or two drops off, ſhewing *a new ſurface in every reſpect like the former*. I do not admit that this is *new cuticle*, ſo quickly regenerated, but the *cuticular ſurface of rete mucoſum*, which has the ſame appearance, and the ſame properties, as the *cuticle.*

Spirit of nitre dropt on the *cuticle,* turns it *yellow,* and produces, though more ſlowly, the ſame effect as the *lunar cauſtic* did.

The *ſubſtance of the teeth,* like the *cuticle,* has been ſuppoſed to have *no veſſels,* though it was *originally* depoſited by *veſſels*; and there are ſeveral circumſtances which favour this opinion; yet in attempting to *ſaw a tooth* in the living body, the patient complained of *pain* the moment the *ſaw* got through the *enamel.* If there are *nerves* in the bony part of a *tooth,* there can be no doubt of its *alſo having veſſels.*

Cartilages covering the ends of bones, in the full grown animal, have not the leaſt *veſtige of veſſel* that can be demonſtrated; but cartilage may be *abſorbed* as well as *bone*; and if in the diſeaſed ſtate, it is moſt probably *vaſcular*, it muſt have been *ſo* in the ſound ſtate.

Having formerly obſerved, that the *brain* ſteeped for months in *ſpirit of ſea ſalt*, inſtead of being *diſſolved* like the *muſcular fleſh*, or like the *viſcera* of the *thorax* and *abdomen*, become *harder* and *firmer*, * I wanted to ſee what effect *concentrated acids* would have upon the *cuticle*.

I took a piece of the *cuticle* of a *child at birth*, and divided it into three por-

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* It not only becomes firmer, but its *fibrous texture* may *thus* eaſily be demonſtrated. The *nitrous acid*, however, diſſolves it *entirely*, and the *vitriolic converts it partly into pulp*.

Not only the *brain*, but the *nerves* alſo, appear to have *other properties* than we have hitherto apprehended. *Some years ago*, I demonſtrated, by *experiments on living animals*, that *nerves divided unite* again; and that when *portions had been cut out*, *they were regenerated*.—In both inſtances the animals perfectly *recovered*.

Theſe experiments I hope ſoon to be able to lay before the *public*; mean time I am happy to find, they have been *recently confirmed* by ſo *great authority*, as that of the *Abbè Fontanà*, to whom I communicated *my diſcovery*, and ſhewed *my preparations of united and regenerated nerves*.

tions;

tions; each of theſe might be about *an inch ſquare*, and were put into ſeparate glaſſes, and ſpread out; two drachms of *vitriolic acid* was poured upon the *firſt*; two drachms of *nitrous acid* upon the *ſecond*, and the ſame quantity of *ſpirit of ſea ſalt*, upon the *third*. After they had remained *an hour* in the *acids*, I found that they were *not diſſolved*; I waſhed them in water, and examined them particularly;—*That* which had been put into the *vitriolic acid*, had, in ſome degree, *loſt its colour*, and was become *browniſh*, but was not however in the leaſt *diſſolved*, and the *proceſſes* were ſtill exceedingly diſtinct; it was not *more tender* to the *touch*, or to *the endeavour* to pull it *aſunder*, than before; nor had it loſt its *elaſticity*, except in a ſmall degree.—*That* which had been put into the *nitrous acid*, though it was not diſſolved, had *ſplit* into *different pieces*, and was more *pulpy* than before, like a piece of *cuticle* from the ſole of the foot, *macerated in warm water*; it was alſo *more tender,* did not bear *handling* ſo well, but the *proceſſes* and *natural texture* of the ſkin were ſtill apparent; it had loſt nearly all *elaſticity.*—*That* on which the *ſtrong ſpirit of ſea ſalt* had been poured, appeared to have *ſuffered leaſt,* and had not even loſt its *colour* in the ſmalleſt degree; it had not loſt its *elaſticity* in any degree; nor was it apparently altered as to its *texture*; the *minute proceſſes* themſelves had undergone no change, and it could be *handled* with as little injury as before; theſe are properties in the cuticle which by no means correſpond with *callous veſſels* or *concreted mucus.* A very remarkable circumſtance in one of theſe experiments was, that though the *nitrous acid* gives the *cuticle* a *yellow colour,* if it touches it while it adheres to the *living body,* it had no immediate effect of this kind upon the *ſeparated cuticle*; nor did I perceive that it was *yellow* till next morning, after it had been *many hours* in water. I repeated *theſe experiments* with *cuticle* ſteeped for an hour in *oil* of *tartar, per deliquium*, leaſt any thing *oily* on the *cuticle* might have prevented the *acid* from getting in *contact* with its *ſurfaces*;—the event was the ſame.

My ſuſpicions that the *cuticle* was *organized* have been ſtill further confirmed, by ſome new obſervations I have made on *ſkin*, injected, with a view to ſhew the appearance of the *ſmall-pox puſtule*. — I have now *more reaſon* to believe, that the *cuticle*, like ſome parts of the *conjunctiva of the eye*, though it cannot be *injected* in the ſound ſtate, was originally *vaſcular*, and circulated the *red blood*. It may ſtill have *veſſels* carrying *tranſparent fluids*, and I would not altogether deny, that *thoſe veſſels* might not ſometimes be again dilated, ſo as to be capable of receiving the *red blood*, or our *injection*. — The *hairs* themſelves, though *reputed* to be *inorganic*, like the *cuticle* and *nails*, or like the *hoofs of animals*, are notwithſtanding ſaid ſometimes to *bleed* ſo as to *endanger life*, in the diſeaſe termed *plica pollonica*.

Though I have not ſeen *veſſels* in *cuticle* or *rete mucoſum*, I have ſucceſsfully injected a *membrane* between *rete mucoſum* and the *cutis*, in the *ſkin of thoſe who have died of the ſmall-pox.*— *This membrane* I diſcovered in conſequence of a converſation I had with *Mr. Baynham, of Virginia*, at the time he ſhewed me ſome preparations of *cutis*, in which, he believed, he had injected *rete mucoſum*. I was ſurprized at the appearance of *veſſels* in *his membrane running parallel to the ſurface of the ſkin, and which formed a net-work*. I was not perfectly ſatisfied however even *then*, that it was *rete mucoſum* he had injected. Mr. *Baynham* was ſo obliging, as to let me have his *preparations home*, and deſired I would examine them carefully, and proſecute the ſubject if I pleaſed.

After ſome time ſpent upon this ſubject, I was ſtill at a loſs what to conclude of his *membrane.* I ſaw that it was *certainly, not rete mucoſum* which I obſerved had already been *previouſly turned down*; and was ſtill adhering to the inner ſurface of the *cuticle.* *This membrane* was much *thicker* than I conceived *rete mucoſum* could be; it was *exceedingly tender and pulpy.* The *ſurface* from which it was removed, as well as its own *internal ſurface, were rough*; nor did the *ſurface* of the *ſkin* appear more *porous* than it was before.—As I could not tell what to make of it, it induced me to make ſome *ſimilar preparations,* and gave me an opportunity of diſcovering *a very beautiful vaſcular membrane in the injected ſmall-pox ſkin,* ſituated in the ſame part with Mr. *Baynham*'s, that is, between the *rete mucoſum* and *cutis.* Mr. *Baynham*'s preparations, he informed me, were made from the ſkin of a leg *where there had been an exoſtoſis of the thigh,* and of conſequence, a *derivation of more* *blood* to the *ſkin* than uſual.—He had plunged the ſkin, after it was injected, into *boiling water*, for a few ſeconds, and afterwards macerated it in *cold water* for ſeveral days.

I had no opportunity of making *experiments* upon *ſimilar ſkin*, but I had many pieces of injected *ſmall-pox ſkin* in *ſpirits*. Mr. *Baynham* had informed me, that he uſed the *boiling water* to *thicken the membranes*, and make them *bear* the *being handled* better. I thought the *ſpirits* would have an equally good effect. I macerated thoſe portions of ſkin in *putrid water* for a week, during the heat of the *ſummer*; the *ſpirits* with which they had been *previouſly impregnated*, made them reſiſt the effects of *this water* longer. *Cuticle* and *rete mucoſum* were already turned down; and upon the *eighth* or *ninth day* I found I could now ſeparate a *vaſcular membrane* from the *cutis*, in which were alſo ſituated the *injected ſmall-pox* *puſtules.* Theſe laſt conſiſted of *circles* of *long floating villi at the circumference,* but of a *white uninjected ſubſtance in the center. This central part Mr. Hunter* had previouſly ſaid, was a *ſlough,* formed by the *irritation* of the *variolous matter.* The ſurface of the ſkin from whence *this membrane* was ſeparated, was *elegantly porous.* The pores *now* appeared *exceedingly more numerous,* and this ſurface of the ſkin was ſtill *tough* and *ſhining.* From the vaſt number of *pores now viſible* I inferred, that the *proceſſes* of the *cuticle* and *rete mucoſum* muſt be alſo more numerous than we are aware of; and many of theſe *proceſſes* muſt be *inviſible* in the microſcope, from their *exility* and *delicate texture,* though their correſponding *pores* are viſible. But as the *proceſſes* of the *larger pores* are viſible to the naked eye; and as Dr. *Hunter*'s *white filaments* are not diſcoverable to the *microſcope,* after they are once tore through, the *inviſible proceſſes* I contend for, *moſt probably* exiſt, and may be the laſt-mentioned *filaments* themſelves.—I macerated the *ſame ſkin* for four or five days more, and ſeparated *another membrane* more delicate than the *former*, but alſo vaſcular; the *former* I eaſily preſerved; the *latter*, attracted by the inſtrument which ſeparated it, or unable to bear the *agitation* of the *water* or *ſpirits* in which it was ſeparated, conſtantly broke down; but the correſponding ſurface of the ſkin was ſtill *tough* and *ſhining*; the *pores* were *now* much *larger* and *more diſtinct* than before, and convinced me that the *appearance was natural*, and that the ſkin had ſuſtained no *real injury* in the *proceſs*.

Was I to deſcribe the *different membranes* which lie on the ſurface of the *true ſkin*, I ſhould *now* ſay they were *five*, each of which I conceive is a *cuticle*, or to become a *cuticle*.

The three firſt, are *evidently cuticles*, and the *two laſt*, moſt probably, are forming into *cuticle*, and, like the *ſecond* and *third*, are to ſucceed the *firſt*, which is perpetually falling off in ſmall portions, *like ſcales*,—the only circumſtance which ſeems to favour *Lewenhoeck*'s doctrine, *that the cuticle is formed of ſcales.*

If I am ſtill not perfectly underſtood reſpecting theſe *five membranes*, I repeat, that *cuticle*, commonly ſo called, makes the *firſt*; the *rete mucoſum* is *double* and makes the *ſecond* and *third*; the *firſt vaſcular membrane* in which the *ſmall-pox puſtules* are chiefly ſeated, makes the *fourth*; and *the membrane*, which may be ſeparated ſome days after the ſeparation of the laſt, by continuing the *maceration*, and which ſhews the *pores ſtill larger*, makes the *fifth*. Theſe *two laſt membranes*, I fancy, might eaſily be detected in the ſkins of thoſe who died of the *meaſles*, *ſcarlet fever*, or other *eruptive diſeaſes*, as well as in the *ſmall-pox ſkin*; for I conceive,

ceive, that theſe *eruptive diſeaſes* do not *create*, but *demonſtrate theſe membranes*, in conſequence of the *great determination of blood*, in theſe caſes, to the *ſkin*.

I mean to proſecute the ſubject, and if any obſervations I may be able to make upon the *ſkin*, will throw any light on *the ſeat of eruptive diſeaſes*, or help *the phyſician* more readily to diſtinguiſh them upon their *firſt appearance*, I ſhall be *ſufficiently rewarded*.

I have within theſe few weeks, procured *portions of ſkin*, under the *ſame circumſtances, exactly*, as *Mr. Baynham*'s.—I have been able to ſeparate a vaſcular tranſparent membrane, ſmooth on both ſides, and more like that which I removed from the *ſmall-pox ſkin* after *cuticle* and *rete mucoſum* had been turned down, than *Mr. Baynham*'s; but, as it has left the ſurface of the *cutis rough*, I am not perfectly contented even with *my own preparations*; they have not eraſed every doubt in my mind reſpecting *Mr. Baynham*'s *membrane*; I am convinced that it is ſomething more than the ſurface of the *cutis* itſelf become tender, from the previous *plunging* it into *boiling water*, and ſubſequent *macerations* in *cold water*, ſeparating into *two layers*.— For I own, it ſeparated of itſelf, without any force, *but* the *roughneſs* of the *ſeparated ſurfaces*, with the *tender pulpy ſtate* of the *membrane*, carry ſome ſuſpicions of a *partly diſſolved* inſtead of *merely ſeparated lamina*. Though I think that *Mr. Baynham* has ſeparated a part of the tender ſurface of the *cutis*, with *his membrane*; yet as the *reticulated appearance* of the veſſels, in the *external ſurface of that membrane* is the *ſame* with that which I have ſeen in the *ſmall-pox membranes*, and in *ſkin*, ſimilar to that which he had prepared, I muſt believe, that one of the *vaſcular membranes* I have ſeen, and *Mr. Baynham*'s are *at bottom, the ſame*; and muſt, therefore, ſtill conſider *him* as the *firſt diſ-*

coverer of the *cuticula quarta*, though I do not admit that *he* has *injected rete mucoſum*. *Mr. Baynham* will do me the juſtice to believe, that could I have talked more favourably of his *preparations*, I would moſt willingly have done it, the *open*, *unſuſpicious manner* with which he treated me, on my viſiting him, and his *frankly* truſting the affair with me, muſt have *pre-engaged* my diſpoſition to oblige him on this occaſion. From the *ſpecimens* I have ſeen of his *anatomical abilities*, and from that *known ardour* with which he purſues his *medical enquiries*, I have no doubt of his becoming *more deſervedly eminent*, than if he had *actually injected rete mucoſum, and* of *courſe done what Ruyſch himſelf could not do.*

Remarks on inſenſible Perſpiration.

Albinus and *Mekel* had both ſuppoſed, that *the perſpirable matter* paſſed through *an imperforated cuticle*, in the form of *vapour*; the *former* ſuppoſed that when it appeared *ſenſible*, in the form of *ſweat*, it was in conſequence of its being condenſed on the ſurface of the body. *They* compared the *oozing* of this *vapour* through the *cuticle*, to the *ſteam of warm water* paſſing through *leather*.

If perſpiration takes place, ſaid I, *through an imperforated cuticle, ſo muſt abſorption from the ſkin.*

Their ideas were *ingenious*, but I could not reconcile my mind to either propoſition.—For *ſweat* is frequently *moſt copious* when there is *leaſt time* allowed for the *condenſation* of the *inſenſible perſpiration*; nor has it been proved that the ſurface of the body is then *colder*, or more capable of *condenſing this vapour*, than at other times.

Though I knew that the *rays of light*, could paſs through *glaſs*, in which there are no *pores*, yet I could not diſcover any reſemblance in *vapour* to *rays of light*, or that *glaſs* was, in any reſpect, like *cuticle*.

Boerhaave obſerved, (as every body elſe muſt have done), that though the *vapour* of the *lungs* in *expiration* was in the *ſummer's heat inviſible*, it became perfectly *diſtinct* when it was condenſed by the *winter's froſt*.

He obſerves, that if the hand is introduced in ſummer into the *powdered ice* of an *ice-houſe*, it *ſmoaks* and gives the ſame appearance as the *breath* does in *winter*: he amuſes himſelf with the *idea* of *winter's cold* being *inſtantly* produced in the midſt of *a ſummer's aſſembly*; each *individual* would then appear, ſays he, like a *heathen deity*, wrapped up *in his own cloud*.

He ſays, that by thruſting the naked arm into a long narrow *glaſs veſſel*,

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the *inſenſible perſpiration* alſo becomes *ſenſible*, in the ſame way that the *vapour of the lungs* becomes *ſenſible* by *winter's cold*, or *breathing* on a *mirrour*. The *cold*, in theſe inſtances, *condenſes the vapour*, as *cold water* thrown round the *worm* of a *ſtill*, does the *ſteam* in *diſtillation*. *He* adds, that it was aſtoniſhing how much *limpid fluid* could, in this way, be collected, but ſays nothing of the *particular quantity*, nor ſeems to have attended to any thing further than the *inſenſible perſpiration*'s becoming *ſenſible*. *Winſlow* ſays, that he could demonſtrate the *inſenſible perſpiration*, by oppoſing his naked head to a *white wall* in a fine ſummer's day; this *vapour*, he ſays, will then become *viſible*, (magnified by the ſun's rays), and appear *aſcending* like *ſmoak*.

I wiſhed to know, whether *this vapour* would become *equally ſenſible* through *leather*; and pervade it, in the manner it was *ſuppoſed* to pervade the *cuticle*.

Sanctorius, in a ſeries of experiments, weighing himſelf daily for *thirty years*, with a view to determine the *quantity* of the *inſenſible perſpiration*, did not take into his *calculation* the *inſenſible abſorption* from the *atmoſphere*; and might *frequently* be attributing that to *checked perſpiration* which belonged to *inſenſible abſorption*. It was alſo thought, that he made the *quantity* of the *inſenſible perſpiration*, in *twenty-four hours*, greater than it *poſſibly could be*. I thought I might be able to come *nearer the truth*, by *weighing* the *actual vapour* of *inſenſible perſpiration*, after it was *condenſed* into a *fluid*.

I wiſhed beſides to know what *affinity* there was between the matter of *inſenſible perſpiration* and the *vapour* of the *lungs*.

The *vapour* of the *lungs* (or *the breath)* was ſaid to be *fixable air and* *water*: others aſſerted, that it *alſo* contained *phlogiſton*.

Having proved *formerly* that the *calces* and *ſalts* of *mercury* were revived into *quickſilver* in the body; and having conſtantly obſerved, that almoſt every thing we eat or drink contains *phlogiſton*; that the *vapour* of the *inteſtines* was *inflammable*; and that there was much *electric fire* in the body: I thought this *latter opinion was more than probable*.

Though many of the properties of *phlogiſton* are *known*, yet as it cannot be procured *uncombined* with ſomething elſe, and in the *ſimpleſt forms* it can be procured, ſeems frequently unwilling to leave the body, to which it is united, to combine with another that may be preſented, unleſs that body is placed with it in ſome uncommon ſituation, ſuch as in a *red heat*, or expoſed to a *powerful acid*, it is *for theſe reaſons* ſtill not *perfectly* known.

Before many readers will underſtand me, it will be neceſſary to premiſe a few obſervations on *fixed air* and *phlogiſton.*

Calcareous earth, burnt in the fire, looſes a *ſomething*, which, when united with *air*, renders it *fixt*, and in conſequence of this, becomes a *calx*, or *quicklime.* Before it was *burnt* it was *inſoluble* in water; now that it is *lime*, water *diſſolves* a certain proportion of it. If *fixt air* is added to *lime-water*, the *lime* which had combined with the water, and was *inviſible*, now attracts the *fixt air*, becomes *calcareous earth* again, and as *inſoluble* in water, is *precipitated* in *fine flakes*, which, for a while, are ſuſpended in the water, and give it a *milky appearance*, but on ſtanding, ſoon fall to the bottom, as a *powder*. If more *fixt air* is added to the water, the water acquires a power of diſſolving *calcareous* *earth*; takes up the powder which it had let fall, and again becomes *tranſparent*; or if, inſtead of adding more *fixt air* to the water, ſome *ſpirit of ſea ſalt* is added, the *calcareous earth* efferveſces with the *acid*; a combination is formed, and the liquor becomes alſo *tranſparent*. *Fixt air* is formed, by the *fermenting* of *ſaccharine fluids* into *vinous*; by the combination of *acids* and *calcareous earths*; the combination of *acids* and *alkalis*; by the *burning* of fuel, and by the *reſpiration* of animals.

It is heavier than *atmoſpheric air*, and depoſited in a veſſel at reſt, remains there for ſome time.

It will not ſerve for the purpoſes of *burning* or *reſpiration*; a *lighted candle* introduced into it is *inſtantly extinguiſhed*; and *an animal*, if he can have no other *air* to breath, *dies immediately*.

Fixable air concentrated, *turns the infuſion of purple or blue flowers red.*

It alſo gives *acidity* to water, and has, for amuſement, been employed inſtead of *lemon juice* to make *punch*.

Metalic bodies, when expoſed to a certain degree of *heat*, loſe *phlogiſton*, and are alſo ſaid to become *calces*.

Phlogiſton is that *principle*, which, in paſſing from ſome bodies, and combining with *air*, occaſions, frequently, *flame*. Thus, *a candle burning* gives over its *phlogiſton* to the *air*, and as *effervescence* is the mark of an *acid* and an *alkali*'s uniting, ſo *inflammation*, or *burning*, is *often* a mark of *phlogiſton*'s *combining with air*.

Many bodies contain *phlogiſton*, and are, notwithſtanding incapable of *inflammation*. *Volatile alkali*; moſt of the *metals* themſelves, are of this claſs.

The metals *calcine* in common air, or part with their *phlogiſton*. Thus *iron*

iron parts with it moſt readily, in what is commonly called *ruſting*, but does it ſo ſlowly, that no *heat* or *inflammation* is perceived.

Metals alſo part with their *phlogiſton* in combining with *acids*; thus *copper filings*, in combining with *nitrous acid*, parts with its phlogiſton and forms *nitrous air*.

Sulphur ſet on fire, parts with its *phlogiſton* to the air, and leaves the other part of its compoſition, the *vitriolic acid*, behind;—ſo does the *phoſphorus of urine*, leaving alſo *its proper acid* behind.

Charcoal contains *it*, in great quantity, *and all inflammable bodies*.

Phlogiſton, united with air, unfits it for *inflammation* of inflammable bodies, or the *reſpiration* of *animals*; a *lighted candle* is extinguiſhed on immerſing it in *this air*; and *an animal* expoſed to it, *dies ſuffocated*.

Phlogiſton, added to ſubſtances, is ſaid to make them *lighter*, and is the only *ſubſtance*, which is *believed* to repel the *centre* of the *earth*.

Some of the moſt eminent *chymiſts* have doubted, however, of this *laſt property*.

With theſe ideas of *inſenſible perſpiration*, *fixable air*, and *phlogiſton*, I made the following experiments.

EXPERIMENT I.

ABOUT ten in the morning, the thermometer at 67° in the ſhade, and 71° in *my apartment*, my pulſe beating 65 in a minute, having taken little or no *exerciſe*, and *feeling* perfectly well, I waſhed and dried my hands, and introduced my *right hand* into a *clean empty bottle*, capable of containing *three pints and a half*. The mouth of the bottle readily admitted my hand; I had previouſly taken a dried bladder, and cutting off the bottom and upper part, had made it into a *hollow cylinder*; this had been *wetted* and drawn on the neck of the *bottle*, like a ſtocking, for ſome way, and was allowed to dry to the glaſs; the middle and oppoſite end were alſo allowed to dry as a *hollow cylinder*, except at that part where it was to be fixed to my wriſt, by a *ligature*. Having made this *ligature*, I obſerved, in *leſs than a minute*, that the inſide of the *bottle* was become *dim*, as it would have been had one held it over the *ſteam of warm water*. In about *ten minutes*, *ſmall drops* began to appear on the *bottom* and *upper ſide* of the *bottle* which was held in the *horizontal poſition*, and equally covered with a *thin wet piece of linen*; *this* was moiſtened from time to time during the experiment, that by the *cold* the *evaporation* from it produced, the *vapour* in the *bottle* might more readily be *condenſed*. After keeping my hand in this ſituation *an hour*, I found I had collected *a tea ſpoonful* of *tranſparent* and *perfectly inſipid fluid.* This *fluid* I poured into the *ſcale* of a balance, which had in its *oppoſite ſcale*, a *weight*, equal to the *weight of a bit of dry ſponge.* With this *ſponge* I abſorbed the *remaining fluid* in the *bottle*, and put it into the *ſcale* with the *former fluid.* The *fluid* I had thus collected *weighed thirty grains.* This experiment I repeated ſeveral times, and in general with the *ſame effect.*

The greateſt part of the fluid was collected by the upper ſide of the bottle, and the vapour *ſeemed* to have a greater tendency *to aſcend.* This, however, might be owing to ſome circumſtance which made the upper ſide of the bottle *colder* than the *under*, though I was not aware of any that would have this effect. A quantity of *lime-water*, equal to the *fluid* collected in the bottle, weighed *thirty-nine grains.*

A *lighted wax taper*, introduced into the *bottle*, at the *end* of the *hour*, before the *fluid* was removed, and immediately on withdrawing my hand, *(which was done very gradually)*, was *not extinguiſhed*, though it burnt *dimly*.

If *my hand* is to the *reſt* of the *ſurface of my body*, as *one to ſixty*; and if every part of that ſurface *perſpired equally with my hand*, then I loſt, during that *hour*, by *inſenſible perſpiration* from the ſkin, *three ounces* and *ſix drachms*; and in *twenty-four hours*, at that rate, would *have loſt ſeven pounds ſix ounces*.

EXPERIMENT II.

I repeated the foregoing experiment ſome hours after, walking gently in open air; at the end of the hour, the *collected fluid weighed forty-eight grains*. This experiment alſo was repeated with the ſame effect. From this I inferred, that the *inſenſible perſpiration* was increaſed *two-thirds* nearly, during *exerciſe*; the *whole ſurface of my ſkin* loſt in *this hour ſix ounces*; and at that rate, *in twenty-four hours*, would have loſt *twelve pounds*. Hard working people, very probably, loſe *ſtill more*.

EXPERIMENT III.

I repeated experiment firſt at nine in the *evening*, thermometer 62°, *the collected fluid weighed only twelve grains*.

The *inſenſible perſpiration*, then, is different under different circumſtances. This quantity, however, was the ſmalleſt I ever obtained in theſe experiments.

The *ſize* of the *body*, the *quantity* of *food* taken in, the *vigour* with which the *ſyſtem* is acting, the *paſſions* of the *mind*, *external heat* or *cold*, are circumſtances which will ever occaſion *conſiderable* *variety* in the *quantity* of the *inſenſible perſpiration.*

EXPERIMENT IV.

I breathed for an hour into the *ſame bottle* which I had formerly uſed for the experiments with *my hand*, and under the ſame circumſtances. *I inſpired freſh air*, and *breathed* into the *bottle*; as I ſuppoſed that ſome of the air of expiration would be returned from the *bottle*, and that of courſe, *all the vapour* would not be *condenſed*, I breathed *more forcibly* into the *bottle* than I would have done in *ordinary reſpiration*. *The proceſs*, eſpecially towards the end, was *exceedingly painful*, and almoſt tempted me to give up the experiment. I believe it was the *ſpoiled air* in the *bottle* that affected me every time I brought my *mouth* to *it* to *expire*. Notwithſtanding of this, I repeated this experiment next day, and with the ſame effect;

that is, *at the end of an hour*, I had collected *a hundred and twenty-four grains of inſipid tranſparent fluid.* The fluid obtained in one of theſe experiments I poured into *lime water*, but it produced *no change* on it. Notwithſtanding the uneaſineſs I felt in breathing into the *bottle*, a *lighted wax taper* introduced into it at the end of the hour, *was not extinguiſhed.*

If I loſt *a hundred and twenty-four grains* of *vapour*, by *reſpiration*, in *an hour*, at the ſame rate, I ſhould have loſt *ſix ounces, one drachm, and thirty-ſix grains, in twenty-four hours*; which, added to the former *cutaneous exhalation*, would make the *whole inſenſible perſpiration* in twenty-four hours, equal to *eight pounds, one drachm, and thirty-ſix grains*; and the *evaporation* from the *lungs*, will be little more than *one fifteenth of the whole.*

Sanctorius ſuppoſing, that a man took into his ſtomach *eight pounds* of *liquid*

liquid and *ſolid* in twenty-four hours, allowed *three pounds* of this to paſs off by *ſtool* and *urine*, and *the other five* he laid to the account of the *inſenſible perſpiration:* the *evaporation* from the *lungs* he calculated at *one-ſixth* of the whole.

It is more than probable, that when the body *weighed heavier* in *Sanctorius*'s experiments, than he expected to have found it; a circumſtance which he attributed to *checked perſpiration*, that *no ſmall part of this weight* was to be laid to the account of *increaſed inſenſible abſorption* from the *atmoſphere.*

I know of no experiments which tend to aſcertain the *preciſe quantity* which is *abſorbed* from the *atmoſphere*; nor do we know whether this *abſorption* is *conſtant* or *periodical.* There are ſome obſervations which prove, that *plants abſorb* from the *atmoſphere*; indeed I cannot conceive, that a body endowed with the property of *abſorbing*, ſhould be conſtantly ſurrounded with *moiſt and fluid air*, and not *abſorb it.*

Sanctorius was thought to have allowed *too much* to the effect of *inſenſible perſpiration*, and the *air* of *Italy* being warmer than *ours*, alone made his *calculation* appear *probable*. He appears alſo to have allowed *too little* out of eight pounds of food for the loſs by *urine* and the *inteſtinal diſcharge*. I have made the *inſenſible perſpiration ſtill more*, and thoſe who do not take one half of *his ſuppoſed quantity* into the ſtomach in twenty-four hours, will think the *propoſition abſurd*. Theſe readers will pleaſe to reflect, however, *that more goes into the body than they know of*; and admitting *inſenſible abſorption* from the *atmoſphere*, appears to me *fully ſufficient* to ſolve this *problem*.

We have inſtances of people's making a ten times greater quantity of *urine* than the *liquids* they *drank*; and I formerly obſerved, that *De Haen* was convinced, that the water of *aſcites* was frequently accumulated by *abſorption from the atmoſphere*. An *eminent philoſopher*, who

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weighs himſelf ſeveral times a day, in a very accurate *ballance*, informs me, that *ſoon after an evacuation* from *a purgative*, he has weighed *ſome ounces heavier* than *juſt before it.*

EXPERIMENT V.

I breathed through *lime water in a curved glaſs tube*, the water immediately became *turbid*; and though on continuing to breath through it, it *once* became *leſs turbid*, yet it never became *tranſparent*, though the breathing through it was continued for *an hour*. On adding ſome *ſpirit of ſea ſalt* to it, it preſently became clear. When *fixt air*, I have ſaid, is added to *lime water*, it becomes *turbid*, but on adding more *fixt air*, the *calcareous earth* is diſſolved, and the liquor becomes *perfectly tranſparent*. There is therefore ſomething elſe in the *air of expiration*, than that *ſomething*, which, added to *air*, makes it *fixt*.

EXPERIMENT VI.

I introduced into *lime water* ſome air, in which a *wax taper* had extinguiſhed itſelf; the *water* inſtantly became *turbid*; no further addition of this air rendered it *tranſparent*; though it became *leſs turbid*, as in the former experiments; and *ſpirit of ſea ſalt*, now added, made it *tranſparent*. This *air*, I own, contains *fixt air*, but it ſurely contains *more phlogiſton*. *Phlogiſticated air* and *fixt air*, it muſt have been obſerved, wonderfully correſpond in ſeveral of *their diſtinguiſhing marks*. They are both *unfit* for *reſpiration* and *inflammation* (or *burning.)* Though they do not *tally* in every circumſtance, may not this depend on the *difference of ſituation*, or ſome difference in the *mode of combination*? A *diluted*, or *weaker acid*, will not produce the effect of a *concentrated one*. The natural *colour* of *ſpirit of nitre* is *yellow*; add a *little water* to it, the *yellow colour* ſtill remains; add a *little more*, it becomes *green*; and add much water it becomes *tranſparent*; all the while it is ſtill *ſpirit of nitre and water.*

EXPERIMENT VII.

I introduced into *lime water* ſome *air*, in which *burning phoſphorus of urine* had *decompoſed itſelf*, and ſhook them together; the *lime water* was inſtantly *decompoſed*; no additions of this *air* made it *tranſparent* again; but on adding ſome *ſpirit of ſea ſalt* it became *tranſparent. The phoſphorus of urine* is allowed by *the chemiſts*, to be the *neareſt* to *pure phlogiſton* of any ſubſtance. This experiment ſeems to prove, that *phlogiſton* will produce the ſame effect on lime water as *fixable air*, and confirms the *ſuſpicion* I have entertained, that *phlogiſticated* and *fixed air are, at bottom, the ſame.*

Dr. Prieſtly found, that the *electric ſtroke* received over the ſurface of *lime* *water*, occaſioned a *precipitation* of the *lime*.

EXPERIMENT VIII.

I repeated experiment firſt, and threw the *fluid* ſo collected into *lime water*, it produced no change in it. I threw ſome *lime water* into the *bottle* where my hand had remained an hour, after *ſome agitation* the *lime water* became *faintly turbid*.

EXPERIMENT IX.

I made a ſimilar experiment to the firſt, with my *foot* inſtead of my hand; of courſe, employed for this purpoſe, a *larger bottle*. The *fluid* collected produced no change on the *lime water*; but *lime water* thrown into the *bottle* and agitated, became *as turbid* as when the *air*, in which the *wax taper* had extinguiſhed itſelf, was mixed with it.

Leaſt the *ſtagnating* of the *perſpirable matter* in the *ſtocking*, and its *fermenting* might be ſuſpected to have *generated* the *fixable air* apparent in this experiment, I muſt obſerve that my *foot* was *previouſly waſhed* in *warm warm.*

This laſt experiment I repeated ſeveral times, and with the ſame ſucceſs: from theſe I inferred, that (admitting the *common theory* of *fixed air* and *phlogiſton*) *ſomething* paſſed off with the *vapour* of *inſenſible perſpiration* by *the ſkin*, which rendered *air fixt*. As this *ſomething*, added to *air*, makes it *heavier* than *atmoſpheric air*, it ſhould have been taken into the *account* of the *weight* of the body in *Sanctorius*'s experiments. If *phlogiſton* paſſes off at the ſame time with the perſpirable matter, along with *that* which, in making *air fixt*, makes it heavier; and if *phlogiſton really repels the centre of the earth,* and *is the principle of levity itſelf*, then, the *one* may *counterbalance* the *other,* and *Sanctorius*'s experiments, as far as *phlogiſton* and *fixed air* are concerned, may be *ſtill* pretty near the truth. If the *re-*

ſpired vapour from the *lungs*, during *exerciſe*, is in the ſame proportion with that from the *ſkin, under that circumſtance*, then the *whole* of the *inſenſible perſpiration* will be *ſtill greater* than I have made it.

EXPERIMENT X.

I introduced my hand, covered with a new *ſhammy leather glove*, into the bottle, as in *experiment* firſt, and under ſimilar circumſtances. In an hour I collected *twenty-four grains* of *inſipid tranſparent fluid*; if the *glove* may be ſuppoſed to have abſorbed *ſix grains*, which is very probable, then I collected nearly the ſame quantity as in experiment firſt.

EXPERIMENT XI.

I introduced my *foot*, with a *rigidly dry boot* on it, into a *large bottle*, and went through a proceſs ſimilar to *experiment firſt*. It was *long* before *any vapour* appeared on the ſides of the *bottle*, but before the end of the *hour* there was *ſome appearance* of *dimneſs*, and *very ſmall drops*.

The *vapour* of *inſenſible perſpiration* then, paſſes through *leather*; and it is very fortunate it does, ſince we may *thus* defend ourſelves againſt the injury of the weather, and *perſpiration continue undiſturbed*. But though *it* pervades *leather*, which is a *dead porous ſubſtance*, yet I cannot believe that it *pervades* in the ſame manner the *living cuticle*. The fluids do not *tranſude*, or *ſoak*, through *living membranes*; nor can I believe that *even vapour itſelf, penetrates an imperforated living cuticle*. The *vapour* paſſed *with difficulty* through the *boot*. It is ſaid, that *dragoons*, who *conſtantly wear boots*, have *ſmall legs*. If *atmoſpheric abſorption* is equally retarded on theſe ſurfaces, their *growth may be prevented*

vented like that of *trees too cloſely planted together*, and from the ſame cauſe. The *preſſure* of the *boot* preventing a *full exerciſe* of the *muſcles*, is alſo to be included.

The *evaporation* from the body, does not appear to me to be like that from *dead matter*. It took place *within the bottle*, nor was in the leaſt *interrupted* though the *air* continued the *ſame* throughout. Were I to compare it to any thing, it would be to the *ſteam* which a *torrent* in falling over the *brow* of a *rock, in its rapidity, flings off in a floating cloud*, while the *torrent* itſelf holds on its way.

The *impetus* of the blood, and the *relaxation* of the veſſels of the *ſkin*, are as certainly neceſſary to the paſſing of the *inſenſible perſpiration*, as to the paſſing of the *ſweat* itſelf.

It may, perhaps, *ſometimes* take place like the *ſweat* in *fainting* or *dying animals*, from *relaxation* of the veſſels only.

If *phlogiſton* paſſes off from the ſurface of the body in *perſpiration*, then, *living animals*, and *bodies on fire*, are, in ſome reſpects, in the *ſame ſituation*, and are both *giving off phlogiſton* to the *atmoſphere*.

That the *blood* contains *phlogiſton* there can be no doubt. That it *burns* with a *blue flame* when dried; that *in this ſtate it revives the calces of metals*, if expoſed with them to a ſufficient heat; that *papers* dipt in the *ſerum* of the *blood* when it happens to be of a *white colour*, *dry greaſy*; that an *oil* is obtained by the *diſtillation* of the *blood*; as well as *Dr. Prieſtly's ingenious experiments* prove this.

That the *blood* parts with *phlogiſton* in *reſpiration* I would alſo admit. I have a ſtrong ſuſpicion that it is *this* which converts the *inſpired atmoſpheric* *air* partly into *fixable air*. Be that as it may, I am convinced there is *ſomething more* in *reſpiration* than the *merely ſeparating phlogiſton from the blood.*

Reſpiration is not only neceſſary to the *free circulation* of the *blood* through the *lungs*, but the *ſtimulus* of the *atmoſpheric air* on the *lungs* has a very conſiderable effect, in *continuing*, and *frequently* in *re-producing* the *heart*'s *motion*.

In preſence of ſeveral of *my anatomical friends*, I opened the *windpipe* of a *dog*, whoſe *ſpinal marrow* had been divided in the *neck*, in whom the *par vagum* and *intercoſtal nerves* had alſo been *divided* at the ſame place. He had been apparently *dead above a minute*, and the *heart* had ceaſed *pulſating*. I introduced a large *blow-pipe* into the opening made in the *windpipe*, and began *inflating* the *lungs*. This I did, in ſuch a way, as to imitate *full* and *ſlow reſpiration*. In about *half a minute*, the *heart* began to *pulſate again*;

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I continued my *inflation* of the *lungs*, and the *heart* continued to beat full equable pulſations, at the rate of *ſeventy in a minute*, for a *compleat half hour.*

The air thrown into the *lungs* was here *phlogiſticated air.* It contained undoubtedly *fixable air*, for, by *one inſpiration* into a large bottle containing *half an ounce of lime water*, on agitating it, I can at any time render the *whole turbid and white as milk.* The *heart*'s *action* was *re-produced* and *kept up* from the *ſtimulus* of the *air* on the *lungs*, and in conſequence of keeping up *their motion.*

I will not *affirm* that it is the *ſtimulus* of *bad air*, which obliges the *muſcles* of the *larynx* to contract ſo as to *ſhut up the glottis*, in *ſuffocation* from *the vapour of* burning charcoal, *&c.* but I *believe* it is *nothing elſe.* I have once or twice been nearly *choaked* from the *ſkin* of a *currant berry*, happening

ing to get between the *root of the tongue* and the *baſis of the epiglottis.* I breathed as in a *fit* of the *aſthma*, and ſaw my *face turgid* with *blood*; this continued till a gulp of water waſhed the *ſkin* away.

My ingenious friend Mr. Crawford, has proved that *atmoſpheric air* contains an *aſtoniſhingly greater quantity of abſolute heat*, than *a mixture of fixed and phlogiſticated air*, or the *air expired from the lungs of animals.* It is probable, that the *heat* which *atmoſpheric air* loſes in the *lungs* on becoming *fixed* and *phlogiſticated*, may give *ſome ſtimulus* to the *ſyſtem*, but there is ſtill *ſomething more* I am perſuaded in *reſpiration.*

From *this long digreſſion on the ſkin, and inſenſible perſpiration*, I once more return to *abſorption in general.*

IV. I ſhall next ſuppoſe, that I am deſired *to give ſome account* of the *manner* *in which abſorption begins, and is afterwards carried on.* *Here*, I muſt own, that many things are *conjectures*. *Leiberkuhn*, whoſe deſcription of the *beginnings* of the *lacteals*, is *ſomething* like mine, ſuppoſes a great part of the *villus* to be of the nature of *ſponge*; he deſcribes the *orifices* which I have ſeen, but makes them *leſs numerous*, obſerving, that it is ſeldom there is more than *one orifice* to the *ſpongy body*, which he calls *ampullula*, and which he conſiders rather as *an appendage* to, than the *beginning* of the *lacteal*. Now, in the *bulbous extremity*, which I have *delineated*, there appeared about *twenty* or *thirty orifices*. Again, he leaves you to ſuppoſe that the *ampullula* as a *ſponge drinks up the chyle*, and that from the *ampullula* the *chyle* is *ſomehow ſqueezed* into the *orifice* of the *lacteal*, which lies behind it.

Haller, in his *Phyſiology*, and *Dr. Fordyce*, in his *Natural hiſtory of the human body*, adopt the *theory originally* ſuggeſted by *Aggiunti*, which compares the *abſorbent veſſel* to a *capillary tube*, and ſuppoſes that *abſorption begins* in conſequence of a *power* in the *veſſel ſimilar to capillary attraction*; that after this, it is carried on by the *muſcular powers* of the *abſorbent*. There are ſome objections to this theory.

In order that *capillary attraction* may take place, nothing more is wanted than *a certain diameter of tube*, and that the *extremity of the tube be immerſed in a fluid*.

Before *animal abſorption* can take place, there muſt be *ſomething more*; there muſt be *a particular ſtimulus* on the orifice of the veſſel, otherwiſe it will *not abſorb*.

Beſides, a *capillary tube* having once taken up *water*, will not take up *oil*, or having taken up *oil*, will not admit *water* to riſe in it, whilſt the *human abſorbents* equally take up *oily* or *watery fluids*.

Mr. Hunter is diſpoſed to conſider the *abſorbent veſſel* in the *light* of a *living animal*, and thinks it may take up *fluids* or *ſolids* as a *leech*, for example, or a *caterpillar*, *take up their food.*

A Gentleman who ſome time ago publiſhed on the *Circulation of the blood*, ſuppoſes that *abſorption begins* in conſequence of *inanition* about the *heart* and *great veſſels*. I am not ſure if I underſtand him *right*; if he means that there is a *vacuum* formed *there*, then *his idea of abſorption* in the *inteſtinal tube*, is ſomething ſimilar to that entertained by the *philoſophers* who adopted the theory of the *fuga vacui*, whilſt *abſorption* on the *ſurface of the body* with him muſt depend upon the *ſame principle* with that by which *water raiſes in a pump*, where not only the *vacuum formed*, but the *preſſure* of the *external atmoſphere* is taken into the account. I do not doubt that the body *abſorbs more* after *evacuations*, and in proportion to the *neceſſity*; but I doubt if there can be any *vacuum* in the *heart* and *trunks* of the *great veſſels.* Theſe parts from their *irritability* and *contractibility*, always adapt themſelves to the *particular quantity*, whether large or ſmall, as we ſee *in bleeding animals to death*, where the *arteries contract*, till *their cavities are quite ſhut up*. If *abſorption* took place, from a *central vacuum*, all parts of the body muſt *abſorb equally*, at the *ſame time*, and there could be *no particular abſorption*, which we know, is not true. A very great abſorption may take place from the *cavity* of the *abdomen*, in the *natural cure of aſcites*, though *very little*, or *none*, is probably going on during that period in other parts.

I think it not improbable that *abſorption may begin ſomewhat in the following way*.

The *abſorbents* have *fibrous coats*, are *irritable* and *muſcular*; *muſcular parts* in general *ſtimulated contract*, and *having contracted*, if in a ſound ſtate, muſt from their own nature be *preſently relaxed*: whatever is to be abſorbed, is applied to the abſorbing ſurfaces, either by the *preſſure* of the *external atmoſphere*; by the *periſtaltic motion* of the *ſtomach* and *inteſtines*; by the *motions of reſpiration*, *pulſation of neighbouring arteries*, or in ſhort, by the *contraction* of *muſcular parts* in general.

This *matter* coming into *contact* with the *orifice* of *an abſorbent*, *ſtimulates* it; the firſt effect of this *ſtimulus* is to make it *contract*; it of courſe takes up *leſs ſpace*, and the *fluid*, or *whatever it is*, ruſhes *forward*; the *abſorbent orifice* now *dilating*, forms a *vacuum*, the *fluid* muſt therefore *ruſh in*, and *ſtimulating* it a *ſecond time*, obliges it to *contract*. This *contraction* not only *propels* what has *entered* the *abſorbent*, but makes room for a freſh quantity to come forward, and in this way, *perhaps*, is the matter to be abſorbed *taken up* from ſurfaces.

I cannot be perſuaded to admit in general *the principles* of *inanimate action* into the *theory* of *living action*. *Mr. Hunter* thinks, the *bones* are evidently conſtructed and adapted to one another on *mechanical principles*, that the *ſame principles* have been conſulted, in the *ſhape*, *diſpoſition* of the *fibres*, and *action* of *muſcles*; but that they take place *no where elſe* in the machine: yet as I find *reſpiration* is carried on, in part, through the *medium* of *a vacuum*, I think it alſo *probable* that *this principle* may have *ſome place* in the *proceſs of abſorption*.

After ſubſtances have once entered the *abſorbent veſſels*, they are *carried forward* in the ſame way as the fluid mounts in the *oeſophagus* of a *horſe* drinking water, or as *liquid fæces* are whirled through the *inteſtines* in *diarrhœa*: I mean, by the *periſtaltic motion* or *muſ-*

cular contraction of the different tubes. The *arteries* themſelves, independant of the *action* of the *heart*, have a *ſimilar power* over the *blood*, and by their *contractions* encreaſe the *velocity* of that *fluid*, and the *force* of the *circulation*.

Haller found that when the *abſorbent veſſels* turgid with their fluids, had *ceaſed*, in the dying animal, *to contract*, by touching them with *oil of vitriol* they could be made to *contract afreſh*, and propelling their contents, became *inſtantly inviſible*.

V. In endeavouring *to prove abſorption* in human bodies, I mentioned *many ſubſtances*, which we knew *had been abſorbed*.

It may be aſked, however, *what are the ſubſtances more commonly abſorbed, and whether there are not very many ſubſtances which cannot be abſorbed?*

The *chyle* and the *lymph* are the fluids *more commonly abſorbed. Boerhaave* ſuppoſes, that during *digeſtion, two pounds of chyle* are every day carried into the *blood.*

This fluid is *white* in *quadrupeds, tranſparent* in birds, in *quadrupeds taſtes ſalt*, ſometimes *coagulates wholly*; ſometimes, like the *blood*, forms itſelf into a *craſſamentum*, and a *thin fluid*, of the ſame colour as the *craſſamentum*, but which *does not coagulate.*

We have no way of eſtimating the *quantity* of *lymph* which is abſorbed. It is *that fluid* which is *ſecreted* by the *extremities* of the *arteries*, on all *ſurfaces*, except the *ſkin*, and into every *cell* of the *cellular membrane.* It is meant to keep theſe ſurfaces and cells *moiſt*, that they may move more eaſily on one another. *It alſo coagulates* when collected from the *abſorbent veſſels*, is generally *tranſparent*, but receives *different tinges* according to the ſubſtances it may be occaſionally mixed with. I am not certain that it ever *coagulates on ſurfaces*, on expoſure to air, and it reſembles conſiderably, when collected from the *abſorbent veſſels*, the *coagulable lymph of the blood*. *Boerhaave* computes, that in conſequence of its *circulating*, a quantity equal to *thirty ſeven pounds* of *this fluid* paſſes through the *heart in an hour*.

Nuck, from experiments made on himſelf, found that *twelve ounces of ſaliva* were ſecreted into the *mouth*, in *twenty-four hours*; the greateſt part of this is moſt probably *abſorbed* with the *chyle* and carried into the *blood*.

The *bile* and *urine* are probably never abſorbed, unleſs they are accumulated in great quantity, and are not carried off by the *uſual outlets*.

An opinion has almoſt univerſally prevailed amongſt *phyſiologiſts*, that the *ſemen*

ſemen was abſorbed from the *teſticle*, and employed to *nouriſh* the body. *Boerhaave* ſays, " indeed it is *ſurpriſing* that a *ſtag*, who has been *caſtrated*, ſhould not in the ſame manner *caſt his horns*, which is a ſtrong argument that *ſomething* returns again from the *ſemen* into the *blood*, capable of *thruſting out or elongating the fibres* and *veſſels*, ſo as to cauſe the *horns* to *fall off* and *grow up again*."

That *eunuchs* have no *beards*, has been uſed as an argument to the ſame purpoſe.

Men living in *celibacy*, are reputed to have *better health* and to *live longer* than *married men*; and *a horſe caſtrated*, is ſaid to be *ſtronger* and to *live longer* than one who has not undergone the *ſame operation*; the *greater ſtrength* and *longevity*, has in theſe inſtances been imputed to *an abſorption of the ſemen*.

The opinion appears to me exceedingly *ill-founded,* from the appearance in the *ſweat* and *urine* we find that *healthy bile abſorbed* in *jaundice,* from *ſtricture* of the *ducts* or *a ſtone* ſticking in *them,* is abſorbed, *only,* to paſs off by another paſſage than the *inteſtinal tube,* and appears incapable of being converted to any other purpoſe than it was *originally* intended for.

If the *ſemen* is not employed for the purpoſes intended by *nature,* it muſt have been perceived that it was *thrown off involuntarily.* I do not ſay, that in obſtructions of the *epidydymis,* it may not be abſorbed to prevent *inflammation* from *diſtenſion*; but I have not found that *ſuch conſtitutions* have been the *ſtrongeſt.*

That there is a *ſtrong ſympathy* between the *head,* the *throat,* and the *organs of generation* in *male animals,* I am firmly perſuaded; and that *removing the organs* may be attended with a *removal* of *thoſe parts* that either are *ſignals of virility,* or intended as *a defence* againſt the other *males of the ſpecies*, who might attempt to *carry off their female*; but that the abſence of the *ſemen* alone would have *this effect*, I doubt much; or that any *other fluid* than the *chyle* is ever employed *to nouriſh the body*.

Any thing in which the *brain* and *nerves* are much occupied, *wears out the body*. *Grief*, *love*, *melancholy*, *intenſe ſtudy*, will do this as well as *venery*; and freedom from any of theſe may have the oppoſite effect.

A conſiderable quantity of *atmoſpheric air* is ſwallowed with our *food*, and probably, partly *abſorbed*. *The air of emphyſema*, I formerly obſerved *was abſorbed*. It is even probable that *atmoſpheric air* may be *abſorbed* by the *ſurface of the body* or *lungs*. *No elaſtic air*, however, is found in any part of the body, except in the *alimentary canal*. From experiments it appears, that *arteries*, or *veins*, cut out in the living body, turgid with *blood*, thrown into water, and placed in the *receiver* of an *air pump*, did not, *on exhauſting the air*, ſhew the leaſt mark of containing *elaſtic air*. Similar experiments were tried on the *gall bladder*, and with the ſame effect.

In a caſe of *ulceration* of the *lungs*, I once exhibited *an ounce* of the *powder* of *ſarſaparilla* daily for ſome weeks; it appeared to have very good effects on the complaint for which it was given; but the *patient* had not taken it *a week*, when he began to complain *that he made bloody water*, and that *there was much ſand in his urine*. He had poured *water* on ſome of the *powdered ſarſaparilla*, and found that it contained *much fine ſand*. I told him I could not believe it *poſſible that the ſand could get into the blood*; that his now paſſing *ſand* was certainly *accidental*, and *unconnected* with the *powder*. He continued the *powder*, but the ſand did not leave the *urine till the powder* *was omitted*; nor could I convince him that the ſand did not get into the blood.

It would ſeem neceſſary, that every thing capable of being *abſorbed* ſhould be *diſſolved*, or *mixed, in liquids*. *Powders rubbed* on the *ſkin*, were there *no moiſture there*, would, generally, *not be abſorbed*. I have ſeen *calomel* lie under the *prepuce*, *unabſorbed*, for ſome days.

Poiſons, the *effluvia*, or the *liquid matter* of *infectious diſeaſes*, we know, *may be abſorbed*. The *venereal diſeaſe*, *bite* of the *mad dog*, the mode of receiving the *jail fever*, and the *inoculated ſmall-pox, afford proofs of this*.

It will not be neceſſary to obſerve, that *cauſtics*, or *concentrated acids*, whoſe effects are to *deſtroy the extremities of the veſſels*, or very *coarſe powders*, even though applied to the *abſorbing ſurfaces* by *liquids*, *cannot be abſorbed*.

It had been *doubted*, whether the *abſorbents* would take up *ſalts*, or very *ſtimulating ſubſtances*; we find, however, *they certainly do*.

If *corroſive ſublimate*, thrown into the *ſtomach*, with a view to cure the *venereal diſeaſe*, was not abſorbed by the *lacteals*, how ſhould it produce a *ſalivation?* The *change* it afterwards undergoes in the body, *in becoming quickſilver*, is moſt probably produced in the *blood veſſels*.

Dr. *Hunter* gives *a remarkable caſe* of *ſome workmen*, employed to clean a *mineral water well*. They had for this purpoſe thrown off their *ſhoes* and *ſtockings*, and gone into the *well*. The *ſalts* in the *water* were *abſorbed* by the *abſorbents* of the feet, and *purged* them all *violently*. I know this has been attributed to the *coldneſs* of the well. *Cold* applied to the *feet* may have *this effect*; but it is difficult to ſuppoſe, that a number of *workmen,* accuſtomed to ſuch employments, ſhould *in this inſtance* be *violently purged,* merely from the *coldneſs* of the *well.*

When *allum* has been exhibited *internally,* as an *aſtringent,* in *hæmorrhages,* with ſucceſs, it is alſo difficult to conceive, that it was *not abſorbed* by the *lacteals*; and that it had the effect of *conſtringing the veſſels of the uterus,* for example, merely by acting on the internal ſurfaces of the *ſtomach* and *inteſtines.*

An ingenious gentleman has informed us, that when he *bathed living rabbits* in a *ſolution of nitre for half an hour,* that *papers* dipt in their *blood* and dried, afterwards *flaſhed* in the flame of a *candle,* and gave evident proofs of the *nitre*'s having gone into the *blood.* Though I do not doubt his *facts,* yet I could not on *ſimilar trials* diſcover that the *blood* contained *nitre.*

The *calces of metals,* may certainly be *abſorbed.* The *palſy* of the *arms,* ſo frequent in *houſe painters,* ſhews this; and demonſtrates from the effects, that the *white lead,* with its *oily vehicle,* had been *certainly,* though *ſlowly, abſorbed* by the ſurfaces of their *hands.*

Did this *poiſon* affect the *hands only,* I ſhould be tempted to attribute theſe effects to *ſympathy,* and believe that the *action* of the *lead* was confined to *theſe ſurfaces* only. But the *conſtipation,* and *painful affection* of the *inteſtines* perſuade me, that it is alſo *abſorbed.*

Boerhaave aſſerted, that the *particles* of the *chyle* were *globular;* that the *orifices* of the *lacteals* were only adapted to the *ſhape* of theſe *globules;* and that in conſequence of this, *no particles* of any other *ſhape* could be admitted into the *orifices* of the *lacteals;* that though the *particles* might be of the *ſame ſhape,* yet if they were *acrimonious,* they *ſtimulated* the *orifice* of the *lacteal*, made it *contract*, or *ſhut itſelf up*, and were alſo in this way *excluded*.

This, *he* conſidered as a very *fortunate circumſtance*, as it prevented any thing *noxious* from getting into the *blood*.

The truth however is, that *very ſtimulating ſubſtances may be abſorbed*.

Few things are more *ſtimulating* to an *irritable ſurface* than *oil of turpentine*; yet we find, that the *abſorbents* of the *ſkin* certainly *take it up*, and that it may afterwards be *ſmelt* in the *urine*.

There can be little doubt that the *abſorbents take up* the *particles* of *powdered cantharides* from a *bliſter plaiſter*. The *ſtranguary*, which ſo frequently takes place, after the application of ſuch a *plaiſter* to the *head*, ſufficiently *proves* this, and ſhews, that *the ſame particles*, accumulated in conſiderable quantity in the *urine,* after having produced *one* inflammation on the *ſkin,* have been able to produce *a ſecond* in the *bladder.*

However *irritating* the *effluvia* of *garlick* are to the *eyes,* its *juice,* rubbed on the *foot,* may afterwards be *taſted* in the *mouth.*

The *aſtringency* of ſome *vegetable decoctions,* does not prevent their being *abſorbed.* Dr. *Alexander* informs us, that he cured an *intermittent fever* by *bathing* his *patient's legs* in a *ſtrong decoction* of the *peruvian bark.*

VI. Our next enquiry ſhall be, *how ſoon after they have been applied to the abſorbing ſurfaces, will ſubſtances, capable of entering the body, generally be abſorbed; and with what velocity do they move forwards after they have been abſorbed?*

That ſome ſubſtances may remain on ſurfaces a *very long time* before they are abſorbed, whilſt others may be abſorbed *almoſt immediately*, we have numberleſs examples.

That the *ſurface* is *naturally* a *good* or *bad abſorbing ſurface*; that it is at *that period diſpoſed* or *not diſpoſed to abſorb*; and that the *ſubſtance* itſelf to be abſorbed, gives *too much* or *too little* or the *proper ſtimulus* to the abſorbing orifice, are circumſtances which will induce *conſiderable variety* reſpecting the *time* in which *ſubſtances may be abſorbed*.

Glyſters ſeldom ſupport a *patient* if he has no other ſuſtenance above *fourteen days*, not only becauſe in the *rectum* and *colon* they are not *ſufficiently animalized*, have not received that *change* which the *ſtomach*, the *mixture* of the *gaſtric juice*, the *ſaliva*, the *bile*, and *pancreatic juice*, produce on our food in converting it into *chyle*, but perhaps alſo becauſe the *former ſurfaces* are not *ſo good abſorbing ſurfaces* as thoſe of the *jejunum* and *ilium*. There are *more ab-*

ſorbents in the *jejunum* than in the *ilium*, and more in the *laſt* and *larger ones* than on the *colon* and *rectum*; a *greater abſorption* is alſo intended to take place from the *former* than the *latter* *.

Mr. Hunter obſerves, that *abſorption of poiſons* ſeldom takes place from an *inflamed ſurface*; a man, for example, is *ſeldom poxed* from the *inflamed ſurface of the urethra* or *glans penis* in *gonorrhea*; *ſuch ſurfaces, he ſtiles bad abſorbing ſurfaces.*

An *ulcerated ſurface*, on the contrary, he obſerves, *begins immediately to abſorb.* No ſooner is a *chancre viſible*, than the *glands* in the *groin begin to ſwell*: an *ulcer* then, in *his ſtyle*, is *one* of the *beſt ſurfaces* for *abſorption*.

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[*] Though I believe that *glyſters ſeldom* paſs the *valve of the colon*, yet I am convinced, from *diſſection* of the *dead body*, they *ſometimes* do, and that, aſſiſted by the *inverted periſtáltic motion* of the inteſtines only, they are, *now and then*, *vomited-up*, ſoon after their exhibition.

That *ſurfaces* are ſometimes *not diſpoſed to abſorb*, appears from obſerving that it is now and then not poſſible to produce *ulceration* by the application of a *poiſonous or infectious matter*, or to *infect* the *body*. Many conſtitutions, without the leaſt precaution, have been expoſed to *venereal virus* for *years*, without being *infected*, and have, however, *been infected at laſt*.

I *inoculated* a *poor woman* and *her child*; the *child* caught the *ſmall pox*, the *woman*, I obſerved, from the *appearance* in the arm, would not have *them*. I *inoculated her a ſecond time*; ſtill the *little inflammation died away* on the *third* or *fourth day*, as it does if one who has had the *ſmall pox* attempts to *inoculate himſelf* again. I concluded ſhe muſt have had the *diſeaſe formerly*, though in ſo *ſlight a way*, as that it had not been obſerved. She attended her child; and in about *ten* or *twelve days* after her child was *recovered*, *ſhe* was taken ill, had a very great quantity of *ſmall pox puſtles*, and narrowly eſcaped with her life.

Mercury, I have already obſerved, will ſometimes *lye on a ſurface*, without being *abſorbed*; at leaſt if it is abſorbed, it is in *ſo ſmall a quantity*, as to produce *no viſible effect* on the *body* or *change* in the *diſeaſe*, for which it was applied, but joined to the *volatile liniment*, or if *friction* is employed, will be *readily abſorbed*.

Subſtances which, by themſelves, would prove *too ſtimulating* to make it poſſible they ſhould be *abſorbed*, may, notwithſtanding, be *ſo managed*, as that *their abſorption* may be procured.

Thus *mercury* frequently proves *too ſtimulating* to the *inteſtines*, and *irritates* their internal ſurfaces; but if joined with *opium*, the *exceſs* of *ſtimulus* may be removed, and the *medicine abſorbed*.

Where *opium* has been attended with *diſagreeable effects* on the *body*, I have known *mercury* given without it, and yet prevented from *ſtimulating* the *ſtomach* and *inteſtines*, by *exhibiting it immediately before* or *after meals*.

I have frequently been obliged to give the *bark* in the *ſame way*. It is ſo apt to *oppreſs* the *ſtomach*, produce *lowneſs* and *faintneſs* when given on an *empty ſtomach*, in ſome habits, that the *patient* is uneaſy till the *primæ viæ* have thrown off their *load* by *purging* or *vomiting*. Given in this way, however, it has *anſwered* pretty well, and *proved* that it was not *neceſſary* to adhere to that *general rule*, of *exhibiting medicines always on an empty ſtomach*.

The *preſence* of *one infectious matter* will *ſometimes prevent the abſorption of another*. A very *curious inſtance* of *this*, I had *laſt ſummer*, in a *child* I had *inoculated* at *Parſons Green*. On the *eighth* *day* after I had *inoculated her*, the *meaſles* appeared, and no change had taken place in the arm where the *variolous matter* had been *inſerted*. It appeared *quite well*, nor could I diſcover where the *puncture* had been made. The *meaſles* laſted their *uſual period*, about *fourteen days*; and on the beginning of the *fourth week* after ſhe had been *inoculated*, the *puncture* in the *arm* began to *inflame*, *formed a fair ſmall pox puſtle*, and the *uſual eruption* followed. The child had them of a very mild kind, though ſhe had been very ill of the *meaſles*, and *recovered*.

A *ſimilar caſe* formerly occurred to *Mr. Hunter*. He mentioned it to me as a *proof* that *two infectious diſeaſes* could not act on the body *at the ſame time*.

It is ſeldom that *a bliſter* produces *ſtranguary* till it has been *applied twelve* or *twenty-four hours*. This makes it *probable*, that the *particles* of the *cantharides* are *not abſorbed* for *many hours* after the *bliſter* has been applied; *perhaps* not till *it* has remained long enough to induce ſome degree of *ulceration.*

The *venereal virus* often lyes *ſix weeks on a ſurface* before it is *abſorbed*; and were it not *then* to produce a *chancre*, might *not be abſorbed at all. Mr. Hunter* obſerves, that this *virus* will lye *many months* in the *vagina* of a woman without being *abſorbed*, or affecting her in the leaſt, whilſt a *proof* of its having been *there*, was, that in that time ſhe had *infected different men.*

Variolous matter lyes commonly *eight*, ſometimes *fourteen days* in the *wound* before it is *abſorbed*; and the *poiſon* of the *mad dog*, *ſix weeks*, *three months*, or *longer* before it produces *hydrophobia*, or has been carried into the blood.

Blood extravaſated into the *cellular membrane*, will ſometimes remain *months there* before it is *wholly abſorbed*; and *extravaſated*

extravaſated under the *nails*, or immediately under the *cuticle*, appears *not to be abſorbed at all.*

The *ſkin* tinged with *gunpowder*, or certain *black juices*, in ſtaining *devices*, on the *arms* of *young people*, (a practice common with *ſailors*,) retains the *device unaltered through life.*

Mr. Hunter obſerves, that the *ſmall pox pits* in the ſkin, which were originally formed during *childhood*, become *larger* in the *adult*.

From *both circumſtances*, it appears *probable*, that unleſs in a *diſeaſed ſtate*, *ſkin is not changed*; but the *pit* becomes *larger* by the *introduction* of *new matter* blended with the *old*. That *new ſkin* after the healing of *ſores*, is always of an *inferior kind*, neither having the *ſame appearance* nor *ſame ſtrength* as *originally formed ſkin*, makes this ſtill *more probable*. It is poſſible that the *brain* itſelf may *alſo* be exempted from the *change* or *renovation* which takes place in *other parts*; and could this be *proved*, might furniſh thoſe *phyſiologiſts*, who *deny* that there are any *lymphatics* in the *brain*, with a *plauſible reaſon* for their not being *there*.

As there is *great variety* reſpecting the *time* in which *ſome ſubſtances* will be *abſorbed*; I believe there is alſo *ſome variety* in the *abſorption* even of the *chyle* and *lymph*.

The *abſorption* of the *latter*, in a *healthy animal*, I believe, is *pretty conſtant*, but I have no idea of its being near *Boerhaave*'s *quantity*, and *ſuſpect* that it goes on very ſlowly.

If the *ſecretion* on *internal ſurfaces* and in *cells*, was of the *ſame nature* with that *vapour* thrown off from the *ſurface* of the *ſkin*, a *mere water*, and in the ſame proportion, from the *preceding experiments*, the *abſorbents* would have a *great deal* to do in *re-pumping* this *fluid* and preventing *dropſy* in *every cavity* and *cell.* Although I do not allow with *Mr. Hewſon*, that the *fluid* of *ſurfaces jellies on expoſure to air*; and though I believe, that a great part of it is ſimilar to the *vapour* of the *lungs* and *ſkin*, yet, I know there is *ſomething more* in its *compoſition*. I ſhould ſuſpect that the *fluid of ſurfaces* was pretty ſimilar to the *liquor pericardii*, and that as there was to be *greater motion* in the *heart* a *greater quantity* of *the ſame fluid* was provided. The *liquor pericardii* is partly *water*, partly *coagulating lymph*, but ſo *combined* with the *water*, that it does not *coagulate*, unleſs *heat* is applied. If I may judge of the *nature* of the *lymph*, from *the chyle*, I ſhould alſo believe, that it did not generally *wholly coagulate*; and that in *weak conſtitutions*, the quantity of *coagulable matter* was in *a very ſmall proportion*. Whilſt, in the ſtronger habit, like the *blood* it might be *more denſe*, and contain *a very large proportion* of the *coagulating lymph*. That the fluid of *internal ſurfaces* is not like *that* which *exhales* from the ſurface of the body, appears, from its *colour* and *conſiſtence.* Were it formed from *mere watery vapour,* I cannot conceive how, in the *heat* of the body, it ſhould ever become a *conſiderable fluid.* The *internal ſecretion* into *cavities* more probably reſembles *ſweat,* where there is not only ſome part of the *thinner vapour* collected by *attraction,* but a great proportion of a *denſe fluid.*

Very little fluid ſeems neceſſary to *lubricate* theſe *internal ſurfaces* of the body; nor does it appear to me neceſſary, that this fluid ſhould be *perpetually changed,* in order to prevent *putrefaction.* The *water* of *aſcites,* after having remained *ten years* in the *abdomen,* in all probability unchanged, *is perfectly ſweet.* A *dead child* may lie *ſixteen years* in the *ovarium* or *abdomen* of its *mother,* without ever becoming *putrid.*

I ſhould imagine then, that the *motion* of the *lymph* through the *abſorbents*, was *in general rather ſlow*. The *lymphatic glands*, through which it muſt ſo frequently *paſs*, induce me alſo to believe *this*. A *drop* of *lymph*, in getting from the *great toe* to the *heart*, has, in my opinion, a *four* or *five times longer journey* to make, than a *drop of venal blood*, ſetting out at the *ſame inſtant* from the *ſame place*; the *windings* and *meanders* the former has to paſs through, are *inconceiveable*; whether you ſuppoſe a *lymphatic gland* to be a *congeries* of *contorted, convoluted veſſels*, or a *collection of cells* communicating with one another, but in *a particular way*.

The *motion* of the *chyle* through the *lacteals*, at *particular periods*, is very *rapid*; but as *this abſorption of chyle* is *periodic*, I ſhould alſo believe, that the *abſorption* of the *lymph*, though *generally ſlow*, *was, at that time, alſo increaſed*.

When a *poiſon*, or an uncommonly *ſtimulating matter, is abſorbing*, I preſume, that the *abſorption* is *then alſo very rapid.* I have ſeen *blotches* on the *ſkin*, ſometimes very *quickly* ſucceed the appearance of a *chancre*.

Though the *arteries* and *veins* are always *full*, yet I do not believe the *abſorbent veſſels are ſo.* I can conceive them, at times, almoſt *collapſed*, at leaſt, in ſome parts of the body. The *abſorption* from the *bones* may, perhaps, be *conſtant*. The *lymph*, in its properties, very much reſembles the *coagulable lymph of the blood*, and it is not *improbable*, that *many abſorbents*, for ſome particular purpoſe, not yet known, may be *pretty conſtantly abſorbing it* from the *internal cavities* of the arteries; for if the *abſorbents* can take up the *red blood* in the *dilated ſtate* of the *artery*, they, probably, take up the thinner *lymph* in its *ordinary ſtate*; on the contrary, unleſs when they are *abſorbing* from the *atmoſphere*, the *abſorbents of the ſkin* may be frequently *collapſed*. The *abſorbents* are hardly *viſible* on the *meſentery*, unleſs at the time the *chyle* is *abſorbing*; whereas the *arteries* and *veins* have *uniformly* the ſame appearance. The *air veſſels* of the *lungs* are as *perfect* before birth as ever *they are after*; and their remaining *nine months collapſed*, during the *fœtal ſtate*, by no means unfits them for *future reſpiration*. The *corpus ſpongioſum*, *glandis penis*, and of the *urethra*, is *nothing elſe* than a *plexus of veins*, which may *occaſionally* be *turgid* with *blood*, or be *perfectly collapſed*, according to *particular ſtates* of the *mind* and *body* in the animal to which it belongs. The *cutaneous veins themſelves*, in the *extremities*, are, in *cold weather*, *almoſt empty*.

When the *periods of abſorption* take place, I am convinced, the *velocity* of the *abſorbed fluids* through their veſſels, is *very conſiderable*.

The *velocity* of the *blood* through the *aorta* of a healthy man, at reſt, ſuppoſing his *pulſe* to be 75 *ſtrokes* in a minute, has been *calculated* to be about *eight inches in a ſecond.* As the cauſes which contribute to *retard* this *velocity* increaſe, in proportion to the *diſtance* from the *heart*; the *velocity* of the *blood* in the *extreme veſſels,* is, perhaps, only *one-half of this.* I know of no *phyſiologiſt* who has ſo much as *conjectured* what the *velocity* of the *chyle* may be immediately after *digeſtion is finiſhed.*

On the *meſentery* of many *quadrupeds* there is *no fat*; the *lacteals* run with the *arteries* and *veins* between *tranſparent membranes.* I had often ſeen them *turgid* with *chyle,* in the living animal, but never till lately thought of *aſcertaining the velocity,* with which the *chyle* was *moving forwards.*

Haller ſays of the *chyle,* That it moves *quickly*; that the *lacteals* juſt *turgid* with the *fluids* he had *obliged* them *to abſorb*, *vaniſhed* from his ſight.

Leiberkuhn would lead us to ſuppoſe the *velocity* of the *chyle* very conſiderable, when he *calculates*, that the *abſorbents* of the *inteſtinal tube only*, are capable of throwing into the *blood*, *twenty-five pints of liquid in an hour.*

I had an opportunity lately of attending to this *velocity* in the *chyle* in a dog, who had been opened for ſome other purpoſe. The *lacteals* appeared *exceedingly numerous*, and *turgid with chyle.* I laid hold of *one* which appeared *running* diſtinctly, without ſending off any *branches*, for *ſeveral inches along the meſentery*. I held it between my thumb and finger, cloſe to the *edge* of the *inteſtine*. The *chyle* between my finger and the *root* of the *meſentery*, *quickly diſappeared*. I let the *interrupted chyle ſucceed*, which as *quickly diſappeared*. Repeating *this proceſs* frequently, and *very quickly*, ſo that as little of the *velocity* of the *chyle* as poſſible might be attributed to the *elaſticity* of the *coats* of a *preternaturally diſtended veſſel*, I found, that the motion of the *chyle* was *then conſiderably greater* than at the *rate* of *four inches in a ſecond*.

VII. We have ſome power over the *circulation* of the *blood*; by *ſtimulants*, we can increaſe the action of the *heart* and *arteries*, and *quicken* the *blood's motion*; by *ſedatives*, we can *diminiſh* the *action* of the *former*, and make the *motion* of the *latter ſlower*. As *abſorption* appears to be of *great importance* in our *machine*, it may naturally be aſked, *if we are capable alſo of encreaſing, diminiſhing, or, what may be ſtill more important, of preventing abſorption?*—In ſome inſtances I think we certainly are.

Stimulating the *heart* and *arteries*, alſo *ſtimulates* the *abſorbents* to *greater action*.

Vomiting is a general ſtimulus, and gives a *ſhock* to the whole machine. In conſequence of exhibiting *emetics*, the water of *aſcites* has ſometimes been removed; and when the *cure* has been effected from the *conſtitution* itſelf, *vomiting* has frequently been the method employed by *nature*, for keeping up and encreaſing the *abſorption*.—*Pus*, formed in an *abſceſs*, has ſometimes been removed by artificial *vomiting*, and the opening of the *abſceſs* been *thus* made unneceſſary.

An *ounce* of *ſalt of tartar*, given in a miſtake for a *drachm of ſoluble tartar*, and where *purging* only was intended, in a caſe where there was a very large *accumulation* of *ſynovia* in the *joint of the knee*, brought on ſevere *vomitings*, which continued for forty-eight hours; and induced ſuch an inflammation of the *ſtomach*, as had nearly proved fatal. During this period, the *patient*, who was naturally *ſtrong*, kept his bed, and drank warm water, thin broths, or tea, only, and in conſtant expectation that the *vomiting* would ceaſe *of itſelf*, had not ſent for the *practitioner* who attended him, and who ſuſpecting nothing of this, had not called. When the *patient* had recovered from the inflammation, and the *vomiting* had ceaſed, *the ſwelling in the knee* was found almoſt entirely *diſperſed*. The *ſubſiding* of the *inflammation* could not *here* be attributed to *reſt in bed*; the *patient* had been *confined above a week*, before he took *the emetic*, and the *ſwelling was increaſing, not diminiſhing*, when the *vomiting* took place.

Where *œdema* has been removed by *rubbing the legs* with *warm oil*, the ſucceſs muſt be attributed to the *friction only*, exciting the *abſorbents* to greater action; the *oil* ſerving no other purpoſe than preventing the *hand* from *chafing* the *ſkin* of the leg.

Mercurial ointment laid on a ſurface, would, I believe, frequently, have little or no effect; and ſtrong and conſtant *friction* is generally neceſſary to effect a *compleat abſorption* of the *medicine*.

Cutting down upon a *diſeaſed part*, by rouſing the *living power* in the *part*, to greater and more healthy action, will frequently produce an *abſorption* (or *removal)* of the part. Thus, *Mr. Hunter* obſerves, that *cutting* upon a *venereal node*, or even *bliſtering* it, will ſometimes occaſion its being *abſorbed*.

Irritating a part alſo *produces* and *encreaſes abſorption* in that part; the *inteſtines wore out* by the *irritation* of long continued *purging*, go into *ulceration*, and portions of their internal ſurfaces *are removed*.

Preſſure, as well as *diſtenſion*, are cauſes of *abſorption* in parts. Inſtances of the *laſt* I have already given, in ſpeaking of *abſorption* from the *bladder* and *tubuli lactiferi.*

Every day we meet with inſtances of the *former.* When the body becomes very *weak,* and the *patient* is long confined to his *bed,* nothing is more common, than that the *inferior parts* of the body not being able to bear the *weight* of the *ſuperior parts,* go into *ulceration,* or are *abſorbed;* as in thoſe *ulcers* which are met with oppoſite to the *great trochanters* of the *thigh bone,* or the *tuberoſities* of the *iſchia.*

Mercury ſeems to have conſiderable *powers* in producing and encreaſing *abſorption,* in diſeaſed as well as ſound parts. We lately heard of its good effects in *procuring* an *abſorption* of *water* from the *brain,* in *hydrocephalus.*

I have ſeen *it* have very great effects in *rheumatiſm.* The *rigidity* and *pain* in the *muſcles* in that *diſeaſe,* are often kept up, I believe, from *adheſions*, formed between the *faſciculi* of *muſcular fibres*; and I ſuſpect that *mercury*, by occaſioning an *abſorption* of *theſe*, ſets the *fibres* at liberty, as well as that by introducing *a new ſtimulus* into the body, it tends to *remove the old one.*

Mercury occaſions an *abſorption* of the *alveolar proceſſes*, as we ſee in thoſe who have undergone *ſalivations*, where the *teeth* appear to be *longer* than they *were*, and ſometimes drop out.

I am now employing *mercurial ointment* in a caſe of *dropſy* of the *abdomen*; the *patient* thinks *ſhe diminiſhes* in *bulk*, and ſays, ſhe has been obliged to take in her *flannel waiſtcoat.*

Mercurial frictions have ſometimes had good effects in diſperſing tumours on the *joint of the knee.*

The choice of *proper periods* in which medicines may be applied to ſurfaces, have been ſaid to have conſiderable effect, in encreaſing the *abſorption* from theſe ſurfaces. We have been informed, that in caſes where *mercurial ointment* could not be introduced into the body in the *evening*, the *practitioner* has ſucceeded, by changing the *time of application* to the *morning*. The *abſorbent ſyſtem* may, from the *refreſhment* of the preceding night's *ſleep*, be then in a *more active ſtate*; and if the body is alſo more liable to receive *infection* in the *morning* than in the *evening*, may *then* take up more readily *infectious matter* as well as *mercury*.

Practitioners often wiſh to *diminiſh* the *abſorption of pus* from a *ſore*, and believe, that the abſorbed matter *induces hectic fever*, falls on the *lungs*, and *deſtroys* the *patient*. I know of no method of *preventing this abſorption*. Its *effects* may be *prevented*, perhaps, by methods which tend to *ſtrengthen* the *ſyſtem* in general. *Mr. Hunter* thinks *too much* is aſcribed to *this abſorption* of *pus*; rather believes, that *pus abſorbed* does very little harm. We ſee the *bile abſorbed* in *jaundice*, notwithſtanding the miſchief attributed to *it*, has no other effect on the body than that of producing *languor* or *drowſineſs*. The *hectic fever*, or the *pulmonary conſumption*, conſequent to *bad ſores*, *Mr. Hunter* attributes to the *irritation* and *weakening effects of the ſore*, and not to the *pus*, as any other *long kept up irritation*, with *confinement*, will produce *the ſame effect*.

When *venereal matter* is *abſorbing* from a ſurface, *this abſorption* may be *diminiſhed* by deſtroying the ſurface by cauſtic, and converting it into a *common* inſtead of a *venereal ſore*.

In this way, the *abſorption* of *poiſons* and *infectious matter*, on their firſt application, may alſo be prevented. Deſtroy a *chancre* by *cauſtic*, if you do not *prevent a pox*, you will, at any rate, make *leſs mercury* neceſſary to the *cure.* If the *wound*, from the *bite of a mad dog*, is deſtroyed by *cauſtic*, *immediately* after the accident, no *abſorption* can take place, and the diſeaſe will always be *prevented.*

Mr. Hunter thinks, that if the *cauſtic* is applied any time within *ſix weeks* after the accident, if the *wound*, having begun to *inflame* again, (for it frequently heals *at firſt* like a common wound) has not yet gone into *ulceration*, the *patient* will be *ſecure.*

The *ſmall pox*, ſhould any circumſtance make the *parent repent* of having *inoculated the child*, may alſo be *prevented* by *cutting out the bit of ſkin infected*, or deſtroying *it* by *cauſtic*, within the *three* or *four* (perhaps within *ſix* or *ſeven)* days after the *inſertion* of the *variolous matter.*

The *natural ſmall pox* or *meaſles* may be *prevented* frequently in families, by *waſhing* the *bedſteads* or *cradles* of thoſe who have been *infected* in *cauſtic alkali.*

The *venereal virus* is *prevented* in this way from producing *gonorrhea* or *lues venerea* itſelf; I mean by waſhing the parts which came in contact with the *poiſon* in a *diluted ſolution* of *cauſtic alkali.*

Thoſe who attend the *Lazarettos*, I am told, *prevent* the *matter* of the *plague* itſelf from *infecting*, by cutting out their *hair* and *waſhing* the *ſurface* of the body, with *black ſoap*; which containing more *alkali* than *finer ſoaps*, *combines* more perfectly with the *mucus* of the *ſkin*, and perhaps with the *animal effluvia* themſelves, and makes them capable of being *waſhed off*.

So much for *abſorption in general.* A more full account of this *abſorbent ſyſtem*, with a *particular deſcription* and *elegant engravings* of the *abſorbent veſſels* and *their glands*, the world may ſoon expect from *Dr. Hunter* himſelf. I now proceed to the *abſorption of calomel from the inſide of the mouth.*

On the abſorption of calomel.

AFTER what has been *premiſed on abſorption,* I hope, the *method* by which you propoſe to convey *mercury* into the *blood,* will be *more generally underſtood.*

Still, however, before you can perſuade *venereal patients* to *receive,* or *practitioners* to *preſcribe, calomel,* in the way you recommend, they may wiſh to be informed of *ſome particulars. They* will naturally aſk, *whether it is probable that levigated calomel, applied to the ſurfaces of the body, will be abſorbed?*

Or, admitting that *ſome ſurfaces may abſorb it,* they may wiſh to know, *whether the ſurface of the mouth is not ſuch an irritable ſurface, that it could not* *bear the application of calomel in your way, of courſe could not abſorb it.*

They may wiſh to know *the nature of that evidence* you bring, in ſupport of the *abſorption of calomel from the mouth*, and of its having, *in this way, cured* the *venereal diſeaſe.*

It is not improbable, that when ſatiſfactory anſwers may have been given to theſe queſtions, they may ſtill continue to aſk you, *what ſuperior advantages appear to be gained, by this abſorption, over the common methods of curing the venereal diſeaſe?*

Some information reſpecting theſe particulars may be collected from the following pages.

Your practice recommends itſelf to *me* from ſeveral conſiderations.

I. That

I.

That *calomel, rubbed on the inſide of the mouth, may be abſorbed,* appears probable from *analogy.*

The *particles* of the *blood* ſeen in the *ſimple microſcope,* are *larger* than *thoſe* of *levigated calomel,* yet, as I obſerved, I have had many opportunities of ſeeing the *abſorbents turgid with red blood.* The *particles of quickſilver,* in the beſt prepared *mercurial ointment,* are, in the ſame *microſcope,* as diſtinct as the *particles of the blood*; yet we all know they are very readily taken up by the *abſorbents of the ſkin.* In a written communication of *Dr. Smith*'s * to you, I obſerve, that about twelve or fifteen years ago, *a medicinal ſnuff* was advertiſed, in *London,* and recommended for the cure of *ſcorbutic* and *cutaneous diſeaſes.* This was tried by a perſon who had ſome complaints

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* *Dr. Smith of Bridge-ſtreet, Blackſriars.*

of the ſame nature; from its uſe his *mouth became ſore*; a *ſalivation*, and other ſymptoms uſually conſequent to a *courſe of mercury*, enſued. In two other inſtances, I obſerve *he* found the application of *mercurius emeticus flavus*, as a *ſternutatory*, was attended with the *ſame effects*. The *white precipitate*, applied in form of *ointment* to the *head*, with a view to deſtroy *certain animalcules*, has been blamed, (with what juſtice I ſhall not pretend to ſay) on account of its having ſometimes produced *ſalivation*. Some months ago, *an officer in the army* applied to me, for *the cure of a gonorrhea*; the *injections* I ordered him, ſeemed to *encreaſe* the *inflammation*; but as I had frequently ſeen *local applications cure gonorrhea*, I ſuſpected the diſappointment might be owing to the *awkward manner of uſing the ſyringe*, and ordered him to introduce a *ſmall bougee*, rolled in *five grains of calomel*, moiſtened in *ſaliva*, evening and morning, into the *urethra*, for two or three inches. We ſucceeded better; but on the *third day*, having got *wet, his head ſwelled*, and *a ſalivation enſued.* Why ſhould not the *particles* of *levigated calomel* be *abſorbed* from the *inſide of the mouth*, as well as thoſe of *divided quickſilver* from the *ſkin?* or of *mercurius emeticus*, from *Schneider's membrane?* or of the *ſame calomel* from the *urethra?* If it ſhall afterwards appear, that *your calomel* is applied to the *orifices* of the *abſorbents, partly* in a ſtate of *ſolution* in the *ſaliva*, the *probability* of its being *abſorbed* from the mouth will be *ſtill greater*, as the *particles* of the *mercury* may then be conceived to be *ſmaller* than *any levigation could poſſibly* make them. The *particles* of *levigated calomel* are not much *coarſer*, I preſume, than thoſe of its *precipitate* by the *volatile alkali*; but this *precipitate*, we are certain, may be abſorbed from the *external ſurface of the body*; why may not the *calomel* be abſorbed from the *inſide* of the *mouth?* *Mr. Hunter* has frequently directed *calomel* to be rubbed on the *ſkin*, along with the *volatile liniment*; and ſeen it have the *effects* of *mercurial ointment*. The *volatile alkali* in the *liniment*, it is true, *decompoſes the calomel*; but ſtill the *precipitate* is a *black mercurial powder*, which, it appears, may be *abſorbed*, and which *acts* as the *calomel* itſelf would have done. This very *powder*, I know, is employed in ſome parts of the *Weſt Indies* as an excellent *dreſſing* to *venereal ſores*, and has every *effect* of *mercury* on theſe *ſores*. It is extremely probable, that, mixed with *ſaliva*, it might make a good *mercurial ointment*; or might be *exhibited internally* with as good effects as the *calomel* itſelf. However this may be, I am well aſſured, that, in order to avoid the *trouble* and ſave the *time* employed in making the *common mercurial ointment*, it is uſual with ſome *ſurgeons* to *precipitate mercury* from the *nitrous acid*, by means of the *volatile alkali*; this *precipitate*, like that from the *calomel*, is alſo in form of *a black powder*, which, after it has been repeatedly *waſhed*, and *dried*, is mixed with *hogs-lard*, and forms *their mercurial ointment*. This is ſaid to have the *ſame*, nay, *better effects*, than the merely *divided quickſilver*. The *earth* of *mercury*, in *mercurius calcinatus*, is one of the beſt preparations of that *metal*, yet known, for *internal uſe*. With ſome conſtitutions, however, it is apt to be *too powerful*, irritating the *ſtomach* and *inteſtines*; why ſhould not the *earth*, got by *precipitation* from an *acid menſtruum*, be *equally efficacious*, at the ſame time that it might, *perhaps*, be *milder* in its *operation?*

My friend, *Mr. Smith*, informs me, that ſince the *firſt publication of this letter* he has affected the *mouth*, and *cured* the *firſt ſtage* of *lues venerea*, by rubbing *calomel*, mixed with the *white ointment*, on the *thighs*.

I have often thought, that too little had been done, by men of *real abilities*, in the way of aſcertaining the *abſolute effects* of the *different preparations of mercury.* I do not mean that *ſurgeons* ſhould *try improbable experiments* with their *patients,* or put their ſafety on a *precarious footing,* when they could put it on *an abſolutely certain one*; but caſes do happen where *experiments* might *be made* with *perfect ſafety* to the *patient*; or the *ſurgeon,* having firſt cautiouſly tried the *effect* of a *preparation* on himſelf, might *afterwards,* finding that it anſwered his *expectations,* recommend it, in the *fulleſt confidence,* to his *patient.* I am led to *this reflection* from *finding ſome practitioners* placing the *greateſt confidence* in two of the *precipitates of mercury* juſt mentioned, *whilſt others* affirm, they are *abſolutely inefficacious.*

II.

The ſurface, on which you propoſe the calomel ſhall be rubbed, is, in its own nature,

ture, a better abſorbing ſurface than, perhaps, any other acceſſible to friction in the body.

I have endeavoured to *prove*, indeed it is now *generally allowed, that every ſurface* in the *body, every cell, abſorbs*; but it appears alſo, that the *abſorbents* are by no means *equally active*, and that, like *arteries* and *veins, they* too, from the application of *ſtronger ſtimuli*, may be *excited* to *greater activity*. The *ſtimulus* of the *chyle* and *lymph* on the *orifices* and *coats* of the *abſorbents*, is, I preſume, the *ordinary cauſe of abſorption*; but the *ſtimulus* of *any ſubſtance, capable of being abſorbed*, may be *equally* a *cauſe* of *abſorption*, nay, may be *ſometimes*, eſpecially if *conjoined* with *another, a more exciting cauſe* than the *common one*. A *porter*, for example, is engaged for two or three days in *rubbing down quickſilver with hogs-lard* in a *mortar*; he works the *peſtle*, the *upper end* is every *now and then ſmeared* with a *little* of the *ointment*; he gets *fetid breath* and *ſore gums*; (*the ordinary effect of mercurial friction*) which ſhews that *abſorption* from the *extraordinary ſtimulus* had taken place from one of the *thickeſt cuticular ſurfaces*, the *palms of his hands*. Though *abſorption* may *thus* take place from *ſuch a ſurface*, yet *in general* the *thinner* the *cuticular ſurface is*, the *cloſer* will the *matter*, to be *abſorbed*, be applied to the *mouths* and *coats* of the *abſorbents*, and *ſtimulating* them *more readily*, will be *ſooner abſorbed*. Thus *venereal matter*, applied under the *prepuce* in *men*, or on the *inſide of the labium* in *women*, gets *ſooner* into the *inguinal glands*, *ſooner* into the *habit*, than if it had been applied to the *outſide* of *either parts*. A *little child*, in her maid's arms, received a *kiſs* from a *girl of the town*, who accidentally paſſed by. The *cuticular covering* is remarkably *thin* on the *edge* of the *lips*, and allowing the *blood* to appear more readily through it, gives them their *greater redneſs*. *A chancre*, on the *projecting part* of the *under-lip*, was the *conſequence* of *this ſalute*; which, in a few days, made its appearance, and reſiſted *every application for a fortnight or more.* At laſt it yielded to *mercury*; and thus ſhewing itſelf to be *venereal*, recalled the circumſtance of the *kiſs*, which, but for this, had paſſed *unnoticed.* Had the *venereal matter* been applied to the *cheek, externally*, it is probable, from what we ſee daily, that it might have lain ſome time without producing *any effect*, and at laſt have been *wiped off*; or at leaſt that it would not have produced a *chancre ſooner* than in a *fortnight*, or *perhaps ſix weeks.*

As *Mr. Hunter* finds, that *venereal ulcers* from the *conſtitution*, (or *ſecondary ulcers)* are not *infectious*, cannot communicate the *venereal virus*, the *truth* of *this caſe* may be *ſuſpected.* I do not pretend to ſay, *what was the nature* of *the ſores in the girl's mouth*, or *how ſhe got them. Mr. Hunter*, himſelf, ſaw the *child's lip*, ſaid that the *ſore* looked *more like a chancre than any thing elſe*, and *its recovering*

recovering gradually from the *uſe of mercury*, leaves little room, in my opinion, for *ſuppoſing* that it was *not a chancre*. If the *poiſon* makes its way more *quickly*, from an *inſide* or *thin cuticular ſurface*, why ſhould not its *antidote do the ſame thing?* Why ſhould not *mercury* get *ſooner* into the *habit* from the *inſide of the mouth* than from the *outſide of the thigh?* That the *inſide* of the *mouth* is a *ſurface better fitted for abſorption than any other within the reach of friction*, may poſſibly be denied by *ſome*.

How can a *glandular, ſecreting ſurface*, might they ſay, a *ſurface conſtantly pouring out*, and which, of courſe, may be preſumed *to waſh off every thing laid on it*, be *a good abſorbing ſurface?* Whatever force this reaſoning may ſeem to have, when applied to *dead ſurfaces*, it muſt loſe its weight with thoſe who reflect, that the *ſurface under conſideration* is a *living one*. For if it is *a good objection* againſt the *fitneſs* for *abſorbing in a ſurface*, that it is a *ſecreting one*; the *very ſame objec-*

tion lies againſt *moſt ſurfaces*, and may be urged againſt the *beſt abſorbing ſurface of the body, the internal ſurface of the inteſtines.*

III.

The abſorption of calomel from the inſide of the mouth, in your method appears, from the teſtimony of many of your patients, undeniable.

For if your patients *rub three grains* of *calomel* every day on the *inſide* of the *mouth*, and it does not *gripe* or *purge*; and if the *common effect of three grains*, taken into the *ſtomach, is*, that it *certainly gripes and purges*; then we muſt *conclude* that the *three grains, given in your way*, have not gone into the *ſtomach*, whilſt their producing *evident effects* on the *diſeaſe*, for which they were *exhibited*, *ſhews*, that they have *certainly got into the habit*, or, in other words, *that they have been abſorbed by the*

the ſurface to which they were applied. One of your patients informed me, that he ſaw you *weigh eight grains of calomel*, that he *employed all this quantity, in your way, at once*; and that he went through *this proceſs three ſucceeding mornings without being ſick, griped, or purged.* On what other ſuppoſition, ſhall we be able to ſay, why theſe *eight grains of calomel* did not *purge or gripe*, but that having been *abſorbed* from the *mouth*, they became *milder in their operation*, in the ſame way as we know *the divided quickſilver becomes milder when abſorbed by the ſkin*. Beſides, *ſince the publication of your book*, an *eminent phyſician*, I am informed, has exhibited *calomel in your way* in a caſe of *elephantiaſis*. The *patient* was ordered to *ſpit out*, whenever the *ſaliva* was ſo much accumulated, as to tempt her to ſwallow it. She was ſoon perfectly *cured* by this method. It muſt be *uncommon obſtinacy*, that can make one ſuſpect, that in *this inſtance* the *calomel* was *not abſorbed* by the ſurface of the mouth.

IV. *If*

IV.

If calomel can be abſorbed in your way, it muſt be the moſt eligible method, becauſe it is leſs apt to irritate the ſtomach and inteſtines, and, by purging, to deſtroy its proper effect, than it would be if its firſt action was to be immediately on theſe parts.

There are, however, *ſome preparations of mercury* which may be taken into the *ſtomach* without *irritating too much*, provided *opium* is exhibited along with them; but there are *many conſtitutions* which will not *bear opium*, and if *mercury* cannot be *exhibited properly* without it, patients poſſeſſed of *ſuch conſtitutions* muſt be *extremely unfortunate*, eſpecially if to the former *peculiarity of habit* is joined *another*, viz. *an antipathy in the ſkin to every thing oily.* How many, *originally vigorous*, *juſtly* deduce their preſent *weak bowels*, and *crazy* *conſtitutions*, from *the miſchief* occaſioned by the *action of mercury* on the *primæ viæ!* Now, whether the *calomel* is *here abſorbed* from *the mouth*, or not; ſuppoſing it *actually* goes into the *ſtomach*; but that the circumſtance of its being given *in a liquid form*, *diffuſes its particular ſtimulus*, or that *this ſtimulus* is *blunted* by its *ropy vehicle* the *ſaliva*, and that *thus only* it becomes *milder* in its operation; ſtill the fact is, that it *really is milder*, and of courſe, as the *medicine* is allowed to be *equally efficacious*, *this method* appears preferable to *any other mode of exhibiting mercury internally.*

With thoſe who believe that the *calomel is actually abſorbed from the inſide of the mouth*, and that it does *not paſs into the ſtomach and inteſtines*, there will be ſtill leſs doubt with reſpect to the *propriety of employing this method* rather than that of giving *calomel*, or indeed *any other commonly preſcribed preparation* *of mercury*, in form of *draught*, *bolus*, or *pill*; for though *theſe preparations* taken into the *ſtomach and inteſtines*, may frequently be *abſorbed from their internal ſurface* as well as *from any other*, yet, from the *greater irritability of theſe ſurfaces*, the *ſtimulus* of the *calomel*, or of *other preparations*, will more probably prove *too powerful*; will produce *ſickneſs*, *griping*, and *purging*, and of courſe occaſion their being *hurled out* of the body before *ſufficient time* has been allowed for their *abſorption*. In this way the *remedy* runs a *greater riſk* of being *entirely loſt*, and of producing as *little effect on the diſeaſe, for which it was exhibited*, as the *Peruvian bark would do on an intermittent*, if, inſtead of *ſtaying in the ſtomach*, it was conſtantly *running off by ſtool*. Or though it ſhould not *actually purge*, yet, from its *particular ſtimulus*, the *digeſtive organs*, with whoſe *ſtate* the *functions* of the body are *ſo much connected*, are more apt to be thrown into *diſorder*; *during which* *period,* the attempts of *Nature* to *relieve herſelf* againſt *any diſeaſe,* if not *altogether prevented,* muſt, at leaſt, be *extremely imperfect. On the contrary,* if *calomel* is rubbed on the *inſide of the mouth,* it is applied to *a ſurface,* which happens to be *alternately* expoſed to *heat* and *cold,* and to *conſiderable friction* in *chewing* our *food* and *cleaning our teeth;* of courſe to *a leſs irritable ſurface,* and capable even of *bearing moderate friction.* The *calomel* will *here* be mixed with the *ſaliva* during the friction, will be diffuſed *over the whole mouth,* and *abſorbed* from the *inſide of the lips, ſurfaces of the tongue, roof of the mouth* and *fauces,* as well as *of the cheeks. Thus,* its *firſt effects* will not be in the way of *ſtimulus* on the *primæ viæ,* but it will be gradually and equally applied to the *general ſyſtem. Dr. Hunter* gives a *remarkable inſtance* of *exceſſive irritability* in the *ſtomach* and *inteſtines,* and of the great advantage gained by being able to introduce the *divided quickſilver* into the *ſyſtem* from *another ſurface*, viz. the *ſurface of the ſkin*, after every attempt to make it be taken up by the *former more irritable ſurfaces* had been *ineffectual*. A *gentleman* who had a *venereal ulcer* in his *throat*, and *nodes* on his *bones*, was paſſing through *London* in his way to *Spain*, with a view of obtaining in that *warmer climate*, and from the *Liſbon diet-drink*, the *cure* which he had *deſpaired of* in his *own country*, and from *mercury*. He had tried *mercury*, *internally*, in *every form*, and in the *ſmalleſt doſes*; but it conſtantly produced *ſevere gripings* and *bloody ſtools*. *Dr. Hunter* prevailed on him to delay his *intended voyage*, and to try the effect of *keeping his chamber*, and *rubbing mercurial ointment on the ſkin*. This, *at firſt*, had the *ſame effect* as the *former trials*; produced, *even in this way*, again the *gripings* and *bloody ſtools*. But, by *wrapping him in flannel*, and confining him to the *more conſtant* and *equal warmth of his bed*, ſo as to take off *that determination*

mination to the inteſtines, which *cold*, applied to the *ſurface of the body*, is apt to produce, and *perſiſting* in the uſe of *gentle mercurial frictions*, he was at laſt *perfectly cured*. If the *divided quickſilver thus* became *milder* in its operation, *why ſhould not the calomel be improved from a ſimilar treatment?*

The *particular ſtimulus of mercury* on the *more irritable ſurfaces* of the *ſtomach* and *inteſtines*, and the effects on the *whole body*, more immediately conſequent to this *ſtimulus*, may be prevented *then*, *by avoiding theſe ſurfaces*, and obliging the *mercury* to find its way into the ſyſtem *through another ſet of abſorbents*. I ſuſpect *it is alſo in this way Art* has been able to introduce *morbific matter itſelf* into the *body*, ſo as to make the *effects leſs hazardous* than *thoſe which Nature left to herſelf more frequently produces*. The advantage, for example, gained over the *natural ſmall-pox*, by *inoculation*, *is*, perhaps, *principally* to be deduced from *this circumſtance*, that the *morbific matter*, in the *natural ſmall-pox*, is applied, in *great quantity*, in *form of vapour*, to *the mouth, noſtrils, lungs, ſtomach*, and *inteſtines*; and *ſtimulating* theſe *irritable ſurfaces* into *great irregularity of action*, at the ſame time that *it is abſorbed* from them, produces, as it were, *a double diſeaſe*: whereas in the *inoculated ſmall-pox*, the *morbific matter is* applied in *ſmall quantity* to a *ſmall part of the ſkin*, produces its effects *gradually*, and falls *equally* on the *whole ſyſtem*. This *mode* of reaſoning does not, I know, correſpond with the *ideas* of the *more eminent modern phyſiologiſts*, who think *particular contagions, like particular acids or alkalis*, are always *the ſame*; and produce *different effects only* in conſequence of the *bodies*, to which *they are applied*, happening *to be different*. They maintain, that *the quantity of morbific matter*, applied to bodies, *ſignifies nothing*; that, ſuppoſing *the thouſandth part of a grain of variolous matter* produces *a hundred* *ſmall-pox*; a *pint* or *a gallon of this matter* would produce *no more*; that, at the *end* of the *ſmall-pox*, there is a *greater quantity of variolous matter in the ſyſtem than ever*, and yet it is *then perfectly harmleſs*, and has loſt the *power of irritating*. They have alſo obſerved, that *patients recover of putrid fevers in hoſpitals, ſurrounded by others, in every ſtage of that diſeaſe*. I have *ſome ſuſpicions*, however, that even *particular contagions* may have *different degrees of virulence*, according as they are *communicated* by *one body* or *another, at one period* of the *diſeaſe* or *another*. I have alſo my *doubts*, whether the *quantity* of *morbific matter applied* is *altogether immaterial*. For if, after *infection*, the *ſymptoms following* depended on the *nature* of the *infection*, *conjoined with the ſtate of the body, in which the infection found the patient*, then the *ſtrongeſt men* ſhould conſtantly have the *mildeſt diſeaſe*; which by no means agrees with daily obſervation. The *plague* produces its effects, *not according* *to the ſtate of the perſon it invades*, but, as *Mr. Hunter obſerves, it brings every kind of body to the ſame ſtate on the firſt attack*; ſymptoms of *putrefaction, in all ſorts of conſtitutions, immediately* taking place. This is by no means the caſe in *common fevers*; but as the *plague* is allowed to be *a fever*, it would look as if *febrile matter*, like *an acid*, be ſometimes *more*, ſometimes *leſs concentrated*; and might, of courſe, produce *greater or leſs effects*. If the *quantity* of *infectious matter* was of *no conſequence* in *giving* or *receiving a diſeaſe*, *cutting out the ſwelled gland* in the *venereal buboe*, or *deſtroying the chancre* on its firſt appearance, *could do no good*, becauſe, though in this way, you prevent *future abſorption*, and *diminiſh* the *quantity* of *venereal matter* that ſhall be applied to the *ſyſtem*, yet as we cannot doubt, that *ſome part* of *this matter* has *already* gone into the *ſyſtem*, *the diſeaſe*, if the *quantity of matter* is of no conſequence, ſhould be *as bad* as if the *buboe* had remained,

and

and the *chancre* had not been deſtroyed. Some of the *moſt eminent modern inoculators* have aſſerted, that when they *inoculated* with the *tranſparent fluid*, found in the *puſtule on its firſt appearance*, they were always *more certain* of the *effect*, viz. That the patient *would be infected*, than when they employed the *matter of a perfectly maturated puſtule*. *They* have alſo ſaid, that when occaſionally the *ripe matter had been uſed*, and the *infection* took place, ſtill the *ſmall-pox* were *later* in appearing, were *more dangerous*, or ſeemed *ſomehow imperfect*. I have no experience of the *former method*, having never employed the *lymph*; but *my obſervations* ſo far correſpond with *theirs*, that, in the laſt *ten* or *twelve* I *have inoculated*, where the *matter employed* was always from the *ripe puſtule*, and inſerted by the *gentleſt puncture*, I ſaw nothing of the *eruption* till the *twelfth or fourteenth day*, though the diſeaſe was *perfectly mild in all of them*, and *two of theſe children* were *inoculated* in *the* *third month.* *Poiſons* in the *ſame animal* ſeem to be *more concentrated* at *one time of the year than at another,* and to depend for their *greater effects* on the *greater action of the veſſels* of the animal *producing them.* The *viper-catchers* are more afraid of the *bite* of *theſe animals* in the *months* of *June and July,* than at *any other period.* The bite of the *young rattleſnake,* is ſaid to be *perfectly harmleſs,* whilſt that of a *full grown one is commonly mortal.* If, in the *firſt method* of *inoculation,* when *long and deep inciſions, inſtead of ſlight punctures, were made,* and when, of courſe, a *greater quantity of variolous matter was applied*; if, in this inſtance, a *greater number* of *ſmall-pox* did not *follow,* the *local effects,* at leaſt, were *frequently terrible*; and *ill-conditioned, tedious ulcers* too often *attacked* the *inciſed parts.* With reſpect to the obſervations, that *patients* have a greater *quantity of variolous matter in* them at the *end* of the *ſmall-pox* than ever was applied to the *ſyſtem before*, and that its effects notwithſtanding are *imperceptible*; alſo, that *ſome patients* recover of *putrid fever*, in *the midſt of putrid vapour*; I have only to reply, that nothing is more evident than *the effect of cuſtom: bodies*, in *time*, may be made to live on *poiſon*; and the *firſt effects* of almoſt every application to the body, are *greater* than the *ſubſequent ones.* That the *variolous matter* ſhould at the *end* of the *ſmall-pox* have no effects on the body, is not more *unaccountable* than that we have not the diſeaſe *twice*, or *twenty times* inſtead of *once*. *Some irritations* having *once* acted on us, *loſe the power* of ever *affecting us again*; and *others*, though they *may afterwards* affect us *again*, yet the conſtitution muſt be *altered*, with regard to them, and muſt have returned to its *former ſtate, before infection*, ere it can be *acted on again*. That *all infectious vapour* is at *firſt* chiefly applied to the *more irritable ſurfaces* of the *ſtomach* and *inteſtines*, and that *its firſt action is upon them*,

them, ſeems more than probable. The *indigeſtion, want of appetite, loathing* of *food,* and *uneaſy ſenſation* in the *ſtomach* and *inteſtines, preceding fever,* may poſſibly ariſe from the *infectious miaſmata* acting *particularly* on *thoſe ſurfaces.* There are not wanting *eminent practitioners* in *phyſic,* who, *from obſervation,* inſiſt, that nothing tends more to deſtroy *putrid fever,* on its *firſt attack,* than *emptying the inteſtinal tube.* The *dyſentery, caught by infection,* is uniformly treated in this way. In the *Weſt Indies,* it is notorious, that in moſt *fevers,* unleſs the *bowels* are kept *open* all the while the *fever* laſts, the patient is certainly loſt; and from this circumſtance it is, that the *cathartic antimony* is *there* held in ſuch high reputation. The *purging* ſometimes preceding the *eruption* of the *ſmall-pox* and *meaſles,* alſo contributes to favour *this idea.*

V.

Your friction of calomel is a leſs tedious, leſs laborious proceſs, than the common one of rubbing mercurial ointment.

To be obliged to rub, with their own hands, *half a dram of mercurial ointment*, for *half an hour every night*, is a labour only to be conceived by thoſe who have experienced it. Nothing is more univerſally complained of. What muſt it be when *half an ounce*, or even *a whole ounce* is *rubbed?* The proceſs, in ſhort, frequently *tires the patient* ſo much, that he gives it over before it is *half performed*; and ſometimes omits it, when it ought to have been done. It requires *conſiderable force*; and, if the *patient* is *very weak*, will, to him, be *labour in vain*. I am perſuaded, that patients frequently *fail of being cured* from theſe very circumſtances. Let us ſuppoſe, *fifteen grains* of *mercurial ointment* equal in effect to *one grain of calomel*. This *calculation*, I preſume, will not be thought unfair by thoſe who have given *both quantities*, on *different occaſions*, to *venereal patients*; and have obſerved, that they could go on with the *calomel*, for *twelve* or *fifteen days*, ſenſibly *gaining on the diſeaſe*: whereas, when *the ointment* was employed, it became neceſſary, long before the end of *this period*, to *increaſe the doſe*, in order to keep up *the firſt effects*. If a *patient*, then, muſt either *rub fifteen grains* of *common mercurial ointment*, or *rub a grain of calomel*, and is allowed, after trying both ways, to chooſe for himſelf; there will be little doubt with reſpect to his *choice*. For though *all ſurfaces abſorb*, and any ſurface may be *ſtimulated* to *abſorb more* than it commonly does; yet, *in general*, *ſurfaces ſeem to tire*, (if one might ſay ſo) and, like *muſcles*, having performed a certain *quantity of work*, *refuſe to do more*. At leaſt, we find, by experience, that *mercurial ointment* is taken up *more quickly* by the *abſorbents* from diffuſing it over *a large ſurface*, or by *changing ſurfaces*. *Mer-*

curial ointment, of courſe, being more *bulky*, and more *viſcid*, than *calomel* and *ſaliva*, will require *a larger ſurface*, and a *longer time*, before it can be *ſufficiently rubbed*; or, in the *common ſtyle*, before it can be *rubbed in.* Now if, according to my *calculation*, *three grains of calomel* have as great an effect on the *venereal virus* as *forty-five grains of mercurial ointment*, and if it would require *half an hour's ſtrong friction* to make theſe *forty-five grains be abſorbed* from the *whole inſide* of the *thigh*, while *three grains of calomel, gently rubbed on the inſide of the mouth*, may be *abſorbed* in *half an hour*, or though it ſhould not be abſorbed in *twelve hours*; ſtill, if it is attended with *no trouble* to the patient, who does not perceive the advantage of *employing calomel* rather than *mercurial ointment.*

VI.

Your method alſo recommends itſelf as a neater and more convenient one, than the rubbing mercurial ointment.

The *friction* of *mercurial ointment* on the *thighs* or *arms*, is, to many people, one of the moſt *diſagreeable things* in the world. *Its leaden colour*, *contraſted* with the *ſkin*, makes it *look dirty*. It has *a particularly offenſive ſmell*, independent of that of the *turpentine*, or *balſam of ſulphur*, which may have been employed *in extinguiſhing the quickſilver*. From the *circumſtance* that *half an hour's friction*, *or even a whole hour's friction*, if ever ſo well performed, will not *commonly* effect a *total abſorption of the ointment*, the *ſkin* is generally left a little *ſmeared* with it, after the *proceſs is over*; as it is *oily*, it does not *evaporate*, or *dry*, *readily*; and will eaſily *ſtick* to whatever it *touches*. *Patients* are obliged to *ſleep* in *flannel drawers*, to prevent the *ointment* from getting *through*, and *daubing the bed-clothes*. They muſt be frequently changing theſe *drawers*; conſtantly *waſhing the ſkin*; or, as *waſhing off the ointment* would *retard* rather than *forward* the *cure*, they muſt remain *dirty* for *ſix* *weeks*, or perhaps *three months* together; and if, *by accident*, any part of the *ointment* gets upon their *linen*, from its *colour* it is very apt to give the *alarm in families*, and lead to *diſagreeable diſcoveries.*

VII.

Your method is better, cæteris paribus, than the common modes of exhibiting mercury internally, in as much as it employs friction.

Thin as the *cuticular ſurface is*, to which the *calomel* is applied, *friction* ſeems by no means *unneceſſary*; and I ſhould doubt much (as I have already ſaid) if the *allowing the powder to lie on the ſurface*, or the *mere keeping it in the mouth for ever ſo long a time*, would be attended with the *ſame ſucceſs*. For though I have no idea that *friction forces the calomel into the orifices of the abſorbents*, yet I am convinced that the *ſtimulus*

mulus of the *friction*, joined to that of the *calomel*, makes them *take up the mercury more certainly*, and *more quickly*. I have obſerved that it was difficult, *on any other principle*, to ſay, *why*, in *dropſical patients*, *frequent frictions* ſhould *ſometimes* bring about the *abſorption* of the *extravaſated fluids* in the *cellular membrane* of the *legs*; add to this, that, *in friction*, the matter to be *abſorbed* is certainly more *cloſely* applied to the *orifices* of the *abſorbents*. There are many *habits* in which *mercurial ointment* would never *eradicate* the *venereal virus*, were it *laid* on the *ſkin* inſtead of being *rubbed on it*.

I have *lately* been informed, that your method, in conſequence of the *friction on the inſide of the cheek*, produced *ulceration*. If there was previouſly a *ſcorbutic affection of the mouth*, or the *friction* was performed *too roughly*, this is, by no means, impoſſible. It had *no effects of that kind* on me, nor have I ever ſeen any inſtance of its having had *this effect* on any one elſe.

I have ſometimes found the *cure* of *venereal ſores at a ſtand*, in caſes where the *friction* had been performed by the hands of *delicate* or *indolent patients*; but, on employing the *ſervants*, and defending *their hands* with *oil-ſkin gloves*, the *mercury* has been brought immediately to the *mouth*, and the *cure*, beyond controverſy, not only *ſecured*, but *much accelerated*. May not the want of this ſort of *friction* in the *ſtomach* and *inteſtines*, and the circumſtance of the *mercury's* being obliged to lie on theſe *ſurfaces* till it is *abſorbed*, render it *probable* that *leſs* of the *mercury is there abſorbed*, and, of courſe, give the *external friction of mercury*, independent of the other conſiderations, the *ſuperiority over its internal uſe*?

Dr. Hunter ſuſpects, that the *friction* of *mercurial ointment facilitates its abſorption*,

abſorption, by *dividing it* ſtill more *minutely*, and reducing its *particles* to ſuch a *ſize*, as makes them *capable of being abſorbed.*

VIII.

Your method appears preferable to the rubbing mercurial ointment, in as much as watery fluids may be preſumed to be more readily abſorbed than oils, have not ſome of the diſagreeable qualities of oils, and that there are ſome reaſons to believe that ſaliva and mercury make a more efficacious compound than mercury and oil.

The *vapour*, or *exhalation* from the *external ſurface of the body*, is either *mere water*, or *ſomething mixed* with *water*; ſo that the body may be ſaid to be ſurrounded with *a watery atmoſphere of its own vapours.* This may be ſuppoſed to *repel*, in ſome degree, the *introduction of oil into the body*, by preventing it from coming in *contact* with the *orifices* of the *abſorbents*. But as *ſaliva* approaches more to the nature of *a watery* than an *oily fluid*, though *both* may be *abſorbed*, one may preſume that the *ſaliva* will be *more readily abſorbed* than *hogs-lard*. That *ſaliva* may be *more readily abſorbed* by the *ſkin* than *hogs-lard*, may be granted me; but that *ſaliva* ſhould be *readily abſorbed* by the *ſame ſurface* which *ſecreted it*, *(the inſide of the mouth)* may ſeem *improbable*. Let it be remembered, *however*, that the *abſorbents* are *intermingled with the arteries*; and that *one ſet of veſſels* frequently *take back* what has been *poured out* by the *other*. Again, *ſaliva*, (or, where that might ſeem *indelicate*, the *ſynovia of cows or ſheep)* will *occaſionally* agree better with the *ſkin* than *oil*. There are ſome *ſkins* which ſeem to have an *antipathy to oil*. A *patient* of mine, from rubbing *half a dram* of the *ſtrong mercurial ointment*, two ſucceſſive nights, on the inſide of the thighs, had an *eryſipelatous inflammation* ſpreading all over the *abdomen*, over the *parts of generation*, and upper parts of the *thighs*. This was followed by *excoriation* of all theſe parts, a *leprous-like cruſt*, and a *thin diſcharge*, with *puffing* in ſome parts of the *ſkin*. The parts ſeemed to me to be *verging faſt* to *mortification*. I was obliged to deſiſt from the *mercury*; to throw in an *ounce and a half* of the *bark* in *ſubſtance daily*; give *a couple of grains of opium every night*; and *dreſs externally with flour*. In a few days he got well of the *inflammation*. I now exhibited the *mercurius calcinatus internally*, with all the *ſucceſs* I could have wiſhed. But as he had *a chancre* about the *corona glandis*, and found it troubleſome to *uncover it* in order to *dreſs it*, I was willing to give him as little to do this way as poſſible; and ventured to introduce *a feather*, *ſmeared with mercurial ointment*, under the *prepuce*, as a *dreſſing* for the *chancre*, *twice a day*. After one night the *eryſipelas* was about to be *re-produced*; nay, the *inflammation* had actually *begun* on the *body* of the *penis*. I again deſiſted from the *mercurial ointment*, went on with the *internal uſe of mercury*, and *ſarſaparilla*; and in a *few weeks* he was *perfectly cured*. In ſome inſtances, *opium* exhibited *internally*, has *alſo* produced *eryſipelas*.

Here it does not ſeem that it was *mercury* which produced this effect; nor *opium*, for none had been given; nor *friction*; for, on the *ſecond trial*, *no friction* had been employed. I could place *the inflammation* only to the account of the *oil*. It may ſeem ſtrange to deduce *an inflammation* from *ſo bland a fluid as oil*, and yet here it will be difficult to find *another cauſe*. *Mr. Payne* *, from whom *my patient* had the *mercurial ointment*, aſſures me he never uſes *turpentine* or *balſam of ſulphur* to facilitate the *diviſion of the quickſilver* in making his *mercu-*

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* *Apothecary in Coventry-ſtreet, Haymarket.*

rial ointment, but, at the beginning, employs for that purpoſe *mutton-ſuet only*. It is poſſible the *hogs-lard* in *mercurial ointment* may ſoon become *rancid*; and that it was not *oil*, but *rancid oil*, which produced *this effect*. *Rancid hogs-lard* undoubtedly *breaks down* the *quickſilver ſooner* than *freſh lard*, and may be *a temptation* now and then to *ſave time and labour*. *Here* this does not ſeem to have been the caſe. *Mutton-ſuet* was here employed on account of its greater *hardneſs*, and the *recent lard* added afterwards. Beſides, if the effects had been owing to *rancidity in the oil*, of which the *mercurial ointment* was made, I am afraid we ſhould ſee them *oftener*. At any rate, if this *oil* is apt to become *rancid*, and in this ſtate is to be introduced into the *blood*, *this very circumſtance* will be a ſufficient *motive*, with the *thinking practitioner*, to prefer *ſaliva*, *ſynovia*, or any other *equally convenient*, but *more healthy*, *vehicle* *.

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* *Though mercury, applied to the ſkin, and paſſing through its abſorbents, appears, generally, to act more mildly on the*

ſyſtem;

Further, Mr. Hunter, from ſome experiments made on *himſelf*, thinks it *probable*, that *every preparation of mercury is diſſolved in the human fluids*, and converted, not only into a *new preparation*, but *conſtantly* into the *ſame kind of preparation*, before it acts on the *venereal virus* in the *ſyſtem*. He takes it for granted, that we cannot *taſte* any thing till it is applied in *ſolution to the tongue*: that *powdered glaſs*, for example, would give *no taſte*, as perfectly *inſoluble in* almoſt any *fluid*; but *quickſilver itſelf*, held a conſiderable time in the *mouth*, *gave at laſt the braſſy taſte*, and muſt, of courſe, have been previouſly *diſſolved* in the *ſaliva*. *Corroſive ſublimate*, *calomel*, *mercurius calcinatus*, treated in this way, *alſo* gave the *ſame taſte*.

ſyſtem; yet, I can alſo conceive ſuch a conſtitution of the ſkin, where this general rule may, from that circumſtance, be reverſed; and that the ſtomach and inteſtines may, now and then, bear the ſtimulus of the mercury better than the ſkin. Some recent obſervations have contributed to confirm this laſt conjecture.

The *human ſaliva*, probably, diſſolves many other *metals* beſides *quickſilver*. *Pure copper* leaves a very *nauſeous taſte in the mouth*; and *gold*, applied but for a *few minutes* to the *tongue*, may be *taſted* long after it has been *ſpit out*. If there is a probability that the *calomel*, during the *friction*, in *your method, is in part diſſolved* in the *ſaliva*; it may, *on this account*, be ſuppoſed, not only to be *more eaſily abſorbed*, but alſo to be applied to the *ſyſtem* directly in its *moſt efficacious form*.

It is not, perhaps, neceſſary that *mercury* ſhould be in a *ſtate of ſolution* in the *body*, in order that it ſhould deſtroy the *venereal virus*. I can conceive *mercury*, very minutely divided, ſtimulating the *ſyſtem*, and acting on the *venereal virus*, merely from *coming in contact* with ſurfaces. *Rhubarb* ſeems to *purge* and *paſs the kidnies unchanged*, as appears from the *yellow tinge* it ſtill gives the *urine*, or *linen* dipt in it. It is more

probable,

probable, however, that ſubſtances, capable of *ſolution* in the *human fluids*, will, upon the whole, have a greater effect than *thoſe* which are *not*.

I was *anxious* to diſcover the *ſtate* in which *mercury exiſted in the human fluids*; and *chemically to demonſtrate*, if poſſible, that *ſaliva* and *ſerum* actually *did* or *did not diſſolve quickſilver*, and its *different preparations*. The probable *methods* of detecting *mercury*, I imagined, were the following:

Firſt, If it exiſted in the *fluids* as *merely divided quickſilver*, I thought I might find it by examining *theſe fluids* (particularly when *dried* and *powdered)* in the *microſcope*, or by expoſing *theſe fluids* in different ſituations to *gold*, *ſilver*, *braſs*, or *tin foil*. I particularly depended on *gold*, *which unites immediately with this metal*, and becomes *white*.

Secondly, If the *mercury* exiſted in *theſe fluids* in the form of a *ſalt* or *corroſion*, I had ſtill ſome expectation of finding it by *precipitating* it from its *menſtruum*, by means of the *volatile* or *fixt alkalis*, which throw down nothing from *common ſerum or ſaliva*, but are known to *precipitate mercury* from *acids*. If *corroſive ſublimate*, for example, is diſſolved in water, on the addition of *volatile alkali*, the *mercury* falls to the bottom in form of a *white powder*, and, on adding the *fixt vegetable alkali*, becomes *rediſh* or *yellow*. If *nitre of mercury* is diſſolved in water, on adding *volatile alkali*, the *mercury* falls to the bottom in form of a *black powder*; but, by adding the *fixed vegetable alkali*, falls in form of a *white one*. If *ſaliva* or *ſerum* contained *mercury* in a ſtate of ſolution, or as a *ſalt*, I therefore thought it probable that they might part with the *mercury* to an *alkali*, or poſſibly even to an *acid*; for the *muriatic acid* will take *mercury* from the *nitrous*, and fall to the bottom in water, as *a corroſive ſublimate* in form of *a white powder.*

Thirdly, I found by experiment, that I could detect *mercury mingled* with certain ſubſtances in the ſtate of *cinnabar, calx,* or *corroſion,* by *burning the ſubſtance* containing *it* on a *red hot iron:* for in this *proceſs* the *mercury* parting with its *acid* or *ſulphur*, and receiving *phlogiſton* from the *heated iron*, is *revived* into *running mercury or quickſilver.*

The *precipitates of mercury* from the *nitrous acid,* and from *corroſive ſublimate* in *calomel* by the *volatile alkali* *, I alſo found by experiment were already in the *ſtate* of *divided quickſilver* and *amalgamated gold,* and would as readily be diſcovered as the *mercurial globules of the ointment*, provided they had undergone *no change* in the *body.*

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* Though *volatile alkali* does not readily part with its *phlogiſton,* and appears therefore *not inflammable,* when expoſed to *fire,* yet in certain ſituations *it is inflammable*; and *here* parted with its *phlogiſton* to the *calces of mercury.*

I have made a great many experiments on this *curious ſubject*, but the *reſult* has not been *ſuch* as to leave it in my power *to conclude any thing poſitively at preſent*. The *field* I found *more extenſive* than I was at firſt aware of; the experiments required *great accuracy*, and *frequent repetition*; and a *very ſmall alteration in* the *mode* of *repeating them* ſometimes led to a *concluſion* very *different* from the *former*; ſo that, after ſome pains, I was ſtill obliged to reſt ſatisfied with *probability*.

I am inclined, however, upon the whole, rather to think with *Mr. Hunter*, that *the mercury* is in the *human fluids* in the form of *a new ſalt*; ſince *gold*, &c. were *not amalgamated* by *theſe fluids*, obtained in ſuch a ſtate as made them be preſumed to be *fully ſaturated* (if one might ſay ſo) *with mercury*. The *gold* was not even *tinged white in* any one place, either when ſteeped in theſe fluids *for hours*, or after they had been *dried, powdered,* and then *burnt* on *red hot iron,* and the *vapours* received through an *inverted glaſs funnel* on the ſurface of *that metal*[*].

The *ſaliva* of a man who had *rubbed in* near *ſeven ounces of the ſtrong mercurial ointment,* and whoſe *mouth* notwithſtanding had been but a few days *ſore,* ſhewed *no marks* of containing *quickſilver.* Nor was I more ſucceſsful in attempting to find it in *the blood* of a perſon who had *rubbed* about *ſix ounces,* though he then *ſpit* but little, nor had any other *ſecretion* been apparently *encreaſed.* The *urine* of a perſon who had uſed *ſtill more* of the *ointment* did not appear to contain *divided quickſilver,* nor was it ſenſibly different from *other urine.*

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[*] In *one experiment* there appeared ſome *white ſpots* on the *guinea,* which, on putting it into the fire, *diſappeared:* theſe were owing, *I believe,* to *mercurial vapours;* but as *this experiment* was not confirmed by *ſucceeding ones,* I was afraid I had been *deceived,* and have left the *point* to be determined by *ſome future trials.*

As the *globules of quickſilver* in the *ſtrong mercurial ointment* were ſtill *viſible* in the *microſcope*, I ſaw no reaſon, ſuppoſing the *mercury* taken into the *blood*, and that it *there* underwent *no change*, why it ſhould not be *ſtill in ſome degree viſible in the microſcope*. I examined the *blood* of *the perſon juſt mentioned* as having *rubbed ſix ounces of the ſtrong mercurial ointment*; it was not in the leaſt *diſſolved*, but was coagulated into a very *firm* and *large craſſamentum*. The *ſerum* had not the *ſmalleſt bluiſh* or *black taint*; the *globules* of the *blood* under the *microſcope* had no *unuſual appearance*; nor was there the leaſt ſemblance of any thing *extraneous* in the *liquid blood*.

Having *dried* a quantity of the *craſſamentum*, and *powdered it*, I *again* ſubmitted it to the *microſcope*, in hopes, that if the *quickſilver really* exiſted *there*, from the circumſtance of *powdering the blood*, the *particles of the quickſilver* might have an opportunity of *running together* perhaps, and of courſe might become more *viſible*; but I found not the *leaſt appearance of quickſilver.*

I ſhook *quickſilver*, in *ſaliva*, in the *ſerum of blood*; allowed it to remain in theſe *fluids many hours*, and even in a *heat equal to that of the human body.* I paſſed *theſe fluids* afterwards *through filtrating paper*; but neither *acid*, nor *alkali*, nor *gold* applied, ſhewed *any marks of the preſence of any thing mercurial in theſe fluids.*

A guinea put into the mouth of a venereal patient in a high ſalivation, and kept there *a whole day*, ſhewed *no marks* of having *touched quickſilver.*

Dr. Garthſhore informs me, that a gentleman, who was taking *corroſive ſublimate only*, had thoſe parts of the *flute* on which he played, which *were ſilver*, indiſputably tarniſhed by *quickſilver.*

In whatever ſtate *mercury* exiſts in the *blood*, I *am convinced* that it always *exhales* from the *body, after its action is over*, in the *form* of *running mercury*; and that it may not only be *decompoſed*, but *ſomehow acquire phlogiſton in the human fluids*. I am very much deceived if I have not *repeatedly* ſeen *gold rings* on the *fingers*, *gold watches*, and *money* in the *pockets*, become *white* and *black* from *corroſive ſublimate*, *calomel*, or *mercurius calcinatus*, *exhibited by the mouth*. I would not therefore infer that the *ſaliva does not diſſolve quickſilver*: the *ſolution* of *metals in human menſtrua*, *may be very different from thoſe in the mineral acids*.

Now, as neither *corroſive ſublimate*, *calomel*, nor *mercurius calcinatus*, in *their own* form, or *mixed with water*, have the *leaſt effect in diſſolving gold*; if ever they acquire *this property*, it muſt be by *being revived into quickſilver*. The *corroſive ſublimate* muſt part with *its acid*, and receive *phlogiſton*, in order to become *quickſilver*; the *calomel* muſt alſo be *decompoſed*, and, loſing its *acid*, muſt, in like manner, *receive phlogiſton* before it can be *revived*; and the *mercurius calcinatus*, without parting with any thing, has only to receive *phlogiſton* in order to its being *again* converted into its *original quickſilver*.

As the *heat* of the *human body*, in a ſound ſtate, is ſeldom above 96° of *Farenheit*'s *thermometer*; as *feveriſh heat itſelf*, is only 108°, and as *mercury* does not *boil*, or become *entirely vapour*, at a leſs degree of heat than 600°; it may ſeem *impoſſible* that it ſhould ever *exhale* from the ſurface of the *human body*. Some of our *anceſtors*, and even *a few* of the *moderns*, are of *this opinion*, though for *other reaſons*; and *hence* the report that *mercury* lies in the body, and has been found in *conſiderable quantity after death* in the *cavities of the bones in venereal patients*. They imagined, *perhaps*,

haps, that though the *quickſilver* was kept divided by the *oil*, with which it was joined, before it entered the body, yet, *on its getting there*, the *oil* became more *fluid*, and gave the *quickſilver globules* an opportunity of *falling down*, *re-uniting*, and *forming larger maſſes*, which by their *weight* forced their way into the *cavities* of the *bones*; *and having once placed them there, they knew of no powers in the machine which could remove them.*

I have never ſeen *any mercury* in the *bones*, nor in any other part, in the *dead bodies of venereal patients*, and am perſuaded that *mercury* does not remain any conſiderable time in the *ſyſtem*.

Dr. Fordyce I think proves, in his *chemical lectures*, that *all the metals* are, at times, in *form of vapour* in the *bowels of the earth*; *he* alſo informed us, that *mercurial globules* had been found *adhering* to the *top* of the *tube of a barometer*, though it had been fixed *immoveable*

able to the walls of the houſe *for many years.*

If ſome *ores of cinnabar* are placed in a very moderate degree of *heat*, the *mercurial globules* will *ſweat out* and form *conſiderable drops* on their external ſurfaces. Though *mercury* requires 600° *to make it boil*, yet *like water* it may perhaps be wholly converted into *vapour* without ever coming *near* the *boiling point*, or even in a *degree of heat* little *more* than *that of the atmoſphere*; the *evaporation* indeed will be *ſlower*, and perhaps require a *conſiderable time* before it is *ſenſible*. *Again*, though *mercury* may require a greater degree of *heat* to convert it into *vapour* when in a *large maſs*, yet when the *attraction of its particles for one another* is in ſome degree *diminiſhed by diviſion*, may it not *then* become *vapour* in a *ſmaller degree of heat?*

IX.

Your method of rubbing calomel looks as if it would be a more expeditious way of giving the mercurial ſtimulus to the ſyſtem, and of eradicating the venereal virus.

One of your patients informed me that he formerly had a *chancre* under the *prepuce*; that *you had removed it* by making him *rub a white powder* on the *inſide* of *his cheek*. The effect of the *firſt friction*, he ſaid, *was*, that his *mouth became ſore* and *his breath fetid* in about *ſix hours after*; that *his mouth continued ſo ſore* that he could not *uſe the friction again* for ſome days; that he only uſed *it four times*, but that it *always* affected *his mouth as at firſt*, (ſo that rubbing *twelve grains* took him up *a month* or more); that *the chancre mended from the moment his mouth became ſore*; and that he got *perfectly cured* from *this quantity*, and *in this manner only*.

This is *a very ſtriking caſe*, but ſtanding *ſingle, proves nothing. One grain of mercury* may do *more* with one man than *fifty* with another. I have been told of *a lady* who was *ſalivated* by taking *a quarter of a grain of calomel only.* But if *your friction ſhall generally be found to have this effect*, it will go a great way *to prove* that the affecting the *mouth* is the *chief thing* in *curing* the *venereal diſeaſe*; that the *quantity* of *mercury* in the *ſyſtem* is of *no importance*, provided *the mouth is affected*; in ſhort, that *acting immediately* on the *mouth* is the *quickeſt* and *beſt method* of *cure*; that there is a kind of *connection* between the *ſtate* of the *ſalivary glands* and the *venereal irritation*; and that *inflaming theſe glands to a certain degree, and for a certain period*, (like *cutting* upon a *node)* will deſtroy the *venereal irritation*; that *calomel* rubbed *on* or *near* theſe *glands* is the *moſt proper ſubſtance* for *producing this inflammation*; and that *much mercury is not neceſſary* *to the cure of the firſt ſtage of lues venerea.* From what I have ſeen of *your practice of late*, however, I am rather at preſent diſpoſed to doubt, that the *affection* of the *ſalivary glands*, by *mercury*, is any further connected with the *venereal virus*, than as it ſhews, as a *mercurimeter* (if the expreſſion may be allowed me) the *degree* of *mercurial ſtimulus in the ſyſtem.*

I have ſeldom found, that *patients* were perfectly *ſafe from a relapſe*, who were cured, without *ſome conſiderable affection of the mouth.* I have ſeen the *mercurius calcinatus* cure the worſt *ſtages* of *lues venerea, without affecting the mouth*, but it is in general a *very ſtimulating medicine*, and requires the aſſiſtance of *opium, a gradual encreaſe of the doſe*, and a *longer period, to confirm the cure.*

The more *delicate* conſtitutions, *may be cured* by a *ſmall quantity* of *mercury*, and a *ſmaller degree of ſtimulus* *on the ſalivary glands*; but *ſtronger* conſtitutions, have appeared to me, to have obtained *no real advantage*, unleſs the *mouth was made ſore.*

Though I *formerly* ſuppoſed, that the *calomel applied to the inſide of the mouth*, might be *abſorbed in half an hour*, and though I believe, that the *friction* makes a *great part of it be taken up during this period*, and at any rate *diffuſes it* more generally over the *ſurface of the mouth*; *I am now perſuaded* that the *calomel* is not *wholly abſorbed* in *twenty-four hours*, that there is a *conſtant abſorption of this powder*, taking place, from the *mouth*, and that *ſome part* is alſo *continually* waſhed off by the *ſaliva*, and applied to the *ſtomach*. In this way a *more conſtant and general ſtimulus is kept up, with leſs irritation, than in any other method.*

X.

My experiments, ſo far as they go, *confirm your practice*. I have not been *a* *mere ſpeculiſt* with regard to the *abſorption of calomel:* though I have had but little opportunity of trying your method in curing *venereal infection,* I have ſeen *enough* to incline me to believe that you are right, and I have endeavoured to *aſcertain the abſorption of calomel from the mouth,* the *great hinge* on which *your method turns.*

That I had never taken *a grain* of any thing *mercurial* in my life, did not *unfit me* by any means from being *a proper ſubject* for *any experiment* reſpecting the *operation of mercury on the body.* I took *three grains of calomel (your doſe)*, and taking it up *by little* and *little,* at the intervals of *five* or *ſix minutes,* on the tip of my tongue, applied it *gradually* to the *inſide of the cheeks, lips, roof of the mouth, gums,* and of courſe to the *body of the tongue itſelf,* till I had ſpent about *twenty minutes* or *half an hour* in this ſort of *friction,* uſing as much *force* as *my tongue* was capable of, and taking care to *ſwallow as little as poſſible,*

ble, and particularly not *to ſpit* during *this proceſs*. After the *ſaliva* had accumulated in ſome quantity, ſtill I continued to *waſh the mouth with it*, and to *detain* the *remaining calomel there* as long as poſſible.

I am diſpoſed to believe that the *encreaſed flow of ſaliva*, providing it is *not ſwallowed* or *ſpit out*, rather *tends* to make the *calomel* be more certainly *abſorbed*; for I have obſerved, that when *calomel* had been ſprinkled as a *dry powder* on a *chancre* under the *prepuce*, great part of it was ſtill to be found *next day* from want of ſufficient quantity of *liquid*, as well as of *friction*, to *apply it* to the *orifices* of the *abſorbents*. I uſed *the tongue* in preference to *the finger*, becauſe I found it was leſs apt to bring *a great flow of ſaliva* into the *mouth*, and of courſe did not oblige me *to ſwallow or ſpit*. After waiting *twenty-eight hours*, I found not the *leaſt inconvenience* from the *friction*;

there was at firſt *a glow* upon my *gums* and *cheeks*, a *braſſy taſte* in my *mouth*, but never any *griping*, *ſickneſs*, or *purging*; *nor did my mouth become ſore afterwards.*

In a day or two after I took *three grains of calomel* in a ſmall quantity of *conſerve of hips* as a *pill*, and ſwallowed it *at once*. *Six hours after* it *griped me ſeverely and purged. The rubbing of the calomel on the inſide of the mouth*, I repeated *three different times*, nor did I find it had *any effects* different from *the firſt*.

Mr. Wells from *America*, one of my *medical friends*, rubbed the *calomel* on the inſide of *his mouth*, without perceiving *more inconvenience than I did*.

I think it *material* that the *purging* ſhould be *avoided*, and that the *medicine* ſhould not *thus* be *loſt*; to accompliſh this, and to ſecure a *total abſorption* of the *calomel* from the mouth, it may be applied in *ſmaller doſes* and at *greater intervals*; inſtead of taking *three grains at once*, might not the patient take *one grain* at *three different times daily*, making the *intervals as long as poſſible*. In this way there would certainly be a greater chance of its being all abſorbed, very little would get into the *ſtomach*, at leaſt it would be ſo ſmall a quantity as would *not ſtimulate the ſtomach or inteſtines*, and would produce *no inconvenience*.

Where the *medicine* purges at firſt, why may not a few drops of *laudanum* be exhibited, previous to the *friction*, and be repeated occaſionally?

The *mercurius calcinatus* uſed in the ſame way as the *calomel*, or even *rubbed on the thigh with ſynovia*, would in all probability have *the ſame effects*; *both* of theſe preparations however, as the *quickſilver*, from whence they are formed, are very little, if at all *ſoluble in watry menſtrua*,

ſtrua, and of courſe, unleſs it is believed that they are *ſoluble in the ſaliva and blood*, may be thought leſs proper medicines for *an animal* whoſe *fluids* are of the *watry kind*. *Water* which had ſtood a long time on *calomel* did not become *black* on adding *volatile alkali*; nor did *mercurius calcinatus* appear in the leaſt *diminiſhed* by having been a great while in *water*.

If *the preparations of mercury*, which are *moſt ſoluble in watry fluids*, are likely to prove the *moſt efficacious*, then *corroſive ſublimate and nitre of mercury*, as *ſalts*, and *perfectly ſoluble in water*, bid faireſt for ſucceſs.

Corroſive ſublimate is one of the moſt *active* preparations of *mercury*. *Van Swieten* believed it *the beſt*; it is *ſoluble* in *ſpirit of wine* as well as in *water*, but ſwallowed by *the mouth* ſometimes produces *the worſt of conſequences*.

Nitre of mercury, or the compound of *mercury* and the *nitrous acid,* (in its fluid ſtate, ſaid to be *Ward's drop.)* is perhaps ſtill *ſuperior* to the former in *efficacy.*

Either of theſe *in powder* would be *too corroſive* probably to admit of being *rubbed;* I ſee no reaſon however why *a ſolution of either in water* might not be *abſorbed* from the *mouth,* or even from the *feet or hands;* and why *theſe ſolutions* ſhould not in this way be *milder* and *more efficacious* than they would be were they applied *directly* to the *ſtomach* and *inteſtines.*

It has been objected *to your method* that it *affects* the *breath,* and will lead as certainly to *a diſcovery,* as the *blue colour* of the *mercurial ointment* on the *linen.*

It is very true, that it might do ſo, were not *bad breath* a *more common accident* than the *other,* and were it not ſo *eaſily concealed* by *chewing ſomething aromatic*, or ſcenting *one's handkerchief.*

It has alſo been ſaid, that *your mercury affected* the *mouth locally*, without acting on the *ſyſtem. This is really trifling! Mercury cures inflammations* very frequently; the more ſtimulating *ſalts* of *mercury, uſed* in an *uncommon doſe, might inflame*, but *calomel* has *no ſtimulus* of *this kind*, and is *perfectly mild.* I have known it lie for *ſome weeks* under the *prepuce*, and induce *no inflammation.*

Thus, *Sir*, I have examined, with all the *attention* and *impartiality my preſent ſituation* would admit of, *the merits* of your *propoſed method of rubbing calomel on the internal ſurface of the mouth. The propoſal ſtruck me at firſt*; I thought it *more than probable you would ſucceed; I am now ſatisfied* that *you have ſucceeded.* It may be objected to you, that *your method is not new; other practitioners* have *rubbed calomel before you. It is poſſible they have;*

they have *ſaid nothing of it*, however, to the world; and *their claiming merit to themſelves now on that ſcore*, can have no other effect on you, than that of *confirming your practice*. Should *more weighty objections* be even found *to lie in practice* againſt *your method*, than thoſe I have taken notice of, (for what method has *no inconveniency?)* you will, *at any rate*, have the *merit* of having ſuggeſted *an ingenious idea*; of having done *your utmoſt* to be *uſeful in your profeſſion*; and (if I may be allowed to preſage) of having *laid the foundation of ſome excellent future practice in ſurgery*.

I am, Sir,

with ſincere regard and eſteem,

your obedient humble ſervant,

WILLIAM CRUIKSHANK.

POSTSCRIPT.

THE preceding Letter, is much longer than I intended; but when I began to turn my thoughts to the ſubject of *abſorption*, I found other things connected with it, crowding upon my mind. By this means, the Letter is enlarged far beyond the limits of my firſt intention, which, I confeſs, were not *ſo fixt* as they ſhould have been; for moſt of the experiments related, were made during the time of printing the former part of the letter, and throughout the whole, I noted down my obſervations, juſt as they occurred to me, and in the very language I ſhould at that time have ſpoken them: by which means, the reader will meet with many *colloquial inaccuracies*, which have eſcaped my obſervation, when thinking intenſely on *ſubjects* of *difficulty* and *minuteneſs*.

It may be ſome extenuation of theſe inaccuracies that the greateſt part of the foregoing ſheets were written at thoſe hours which are commonly devoted to *ſleep*, as my time during the day was neceſſarily engaged at *Windmill Street*.

I have had, therefore, no leiſure to correct the Letter as I could wiſh.

As to the *Italics*, which are every where ſpread over the work, and give it a *motley appearance*, I have only to ſay, That the little time I was maſter of made me ſuſpect that I ſhould not be ſufficiently underſtood. If a man writes well, he will always be underſtood, without *Italics*; but it was *diffidence*, and not *affectation*, which made me, imperceptibly, fall into a practice, which, on maturer reflection, I now perceive is *abſurd*.

Having ſaid thus much in excuſe for the manner of the Letter, I am afraid, a few words will be neceſſary alſo in defence of the matter; eſpecially to thoſe who think, that no *new opinions* in ſcience ſhould be publiſhed, till they have been *long weighed* and *attentively conſidered.*

To thoſe, who love their own fame and reputation better than they do the ſcience they cultivate, *haſty communications* will appear very wrong; but to thoſe who think only of the improvement of ſcience; and who would be happy, at any time, to give up their moſt favourite ideas, for new ones better founded, this Letter will, I hope, need leſs excuſe.

The publication of new opinions in any ſcience have unqueſtionably this good effect:—They eſtabliſh facts on a firmer baſis. For if the new opinions are ill founded, the detection of their errors, ſerves to confirm the truth of the old opinions :—If they are well founded, they advance ſcience, and root out error, and it is by theſe means, that we approximate to perfection.

I know men of *real merit*, whoſe *former valuable obſervations*, have in time eſcaped their own memories, and been for ever loſt to the public, from an exceſs of delicacy in publiſhing their opinions to the world.

Every reſpect is undoubtedly due to the public; no man ſhould come before them negligently and unprepared: but if his intentions are the improvement of uſeful ſcience, the good-natured part of mankind will forgive the want of ornament, where they find information.

In order to convince my readers that I am not too tenacious of my opinions, if, on reviewing them, there appears the leaſt room to doubt, I beg leave to make one remark more.

I have ſomewhere ſaid in this Letter, that *calcareous earth*, expoſed to the action of fire, in becoming *quick lime*, gave over *ſomething* to atmoſpheric air, which converted it into *fixable air*.

The *chemiſts*, in all probability, will not allow this; but will ſay, that calcareous earth, in becoming quick lime, gives over to the atmoſphere *fixt air* already formed. I own, that vitriolic acid, and calcareous earth, *efferveſce in vacuo*, and yield *fixt air*, without the aſſiſtance of atmoſpheric air: but I alſo believe, that atmoſpheric air, by receiving *ſomething* from burning bodies, becomes fixt air; and I ſuſpect, that calcareous earth, in becoming quick lime, from the action of fire, not only yields *fixt air, already formed*, and which made a part of its compoſition, but alſo gives off *ſomething*, which, united to atmoſpheric air, makes it *fixt air*.

Atmoſpheric air paſſing into the lungs, in inſpiration, is returned principally fixt air in expiration. I preſume, that it is the ſame air which laſt entered the lungs in inſpiration, which is immediately after returned in expiration, though altered as to its quality. There is one circumſtance, I confeſs, which might be adduced, to ſupport an opinion, that the laſt inſpired air was detained in one ſet of *air cells* in the lungs, and that already prepared fixt air was returned from another ſet of *cells* in its ſtead. The circumſtance I allude to is, that the lungs in the dead body (though expiration is the laſt action of life) always retain more air than is given out at any one expiration.

It is more probable, however, that the ſame air which was laſt inſpired, is immediately expired, though changed in quality. The air in the bottle, in which I kept my foot an hour, appeared on the trial with lime water to be fixt air, though it did not ſeem to have acquired any additional bulk during that period.

I ſuſpect that it is a particular combination of *phlogiſton* and atmoſpheric air which forms fixt air. The experiment in which the air became fixt by the burning of *phoſphorus of urine*, (the idea of which was ſuggeſted to me by *Dr. Keir)* ſeems to prove this.

The *phoſphorus of urine* contains *phlogiſton*, and a very fixed acid. In burning, it therefore gives over the *pureſt phlogiſton* to the atmoſphere. As *phlogiſton* joined to atmoſpheric air produces the ſame effect on lime water as fixt air, I am led to ſuſpect that fixt air, however obtained, is a combination of atmoſpheric air and *phlogiſton*, or of *ſomething*, in ſome reſpects, agreeing with *phlogiſton*. Should this opinion appear to be founded on wrong principles by thoſe whoſe peculiar profeſſion lead them to *chemical experiments*, I ſhall very readily give it up.—Every man has a regard for his own opinions; but I hope I have ſtill a greater regard for *truth*.

ERRATA.

Page 9, line 13, *after* your, *inſert* doctrine of the.—P. 9, l. 20, *for* abſorb, *read* abſorbs.—P. 12, l. 21, *for* begun. *read* began.—P. 16, l. 9, *for* Aſelius, *read* Aſellius.—P. 19, l. 6, *for* taking, *read* finding.—P. 23, l. 12, *for* at theſe times, *read* in the diſeaſed ſtate at leaſt —P. 28, l. 13, *for* porii, *read* pori.—P. 31, l. 18, *for* uniſone, *read* uniſon.—P. 31, l. 24, *for* uniſone, *read* uniſon.—P. 41, l. 3, *dele* which.—P. 42, l. 9, *for* is, *read* are.—P. 43, l. 19, *for* Aſelius, *read* Aſellius.—P. 45, l. 9, *for* ſimular, *read* ſimilar.—P. 46, l. 24, *before* taught, *inſert* firſt.—P. 48, l. 5, *for* venticulorum, *read* ventriculorum.—P. 52, l. 6, *after* demonſtrate, *inſert* it.—P. 53, l. 7, *for* I will not ſay that the cuticle does not terminate, *read*, I have ſaid the cuticle does terminate.—P. 56, l. 8, *for* menſenteric, *read* meſenteric.—P. 57, l. 18, *for* there, *read* theſe.—P. 65, l. 14, *for* Ruyſche's, *read* Ruyſch's.—P. 75, l. 23, *after* freedom, *inſert* in the villi.—P. 90, l. 20, *dele* not.—P. 91, l. 3, *for* Pollonica, *read* Polonica.—P. 91, l. 15, *for* his, *read* this.——P. 92, l. 3, *for* his, *read* this.——P. 99, l. 18, *for* Buyſch, *read* Ruyſch.——P. 99, l. 21, *for* Mekel, *read* Meckel.—P. 103, l. 7, *for* be attributing, *read* attribute.—P. 107, l. 4, *for* metalic, *read* metallic.—P. 108, l. 1, *dele* moſt readily.—P. 108, l. 7, *dele* filings.—P. 111, l. 20, *for* would, *read* could.—P. 117, l. 19, *for* a ten times, *read* ten times a.—P. 118, l. 2, *for* ballance, *read* balance.——P. 122, l. 4, *for* warm, *read* water.—P. 132, l. 19, *for* raiſes, *read* riſes.—P. 139, l. 17, *for* a horſe caſtrated, *read* a horſe *not* caſtrated.—P. 139, l. 19, *dele* not *and* ſame.—P. 145, l. 5, *for* allum, *read* alum.—P. 147, l. 18, *for* ſtranguary, *read* ſtrangury.——P. 149, l. 24, *for* ilium, *read* ileum.—P. 150, l. 1, *for* ilium, *read* ileum —P. 152, l. 1, *for* puſties, *read* puſtules —P. 152, l. 10, *before* will, *inſert* it.—P. 154, l. 11, *for* puſtle, *read* puſtule.—P. 154, l. 20, *for* ſtranguary, *read* ſtrangury.—P. 160, l. 7, *for* a four or five times, *read* four or five times a.——P. 166, l. 14, *dele* a.